MEMOIRS OF THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER.

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OF THE

LITERARY AND PHILOSOPHICAL

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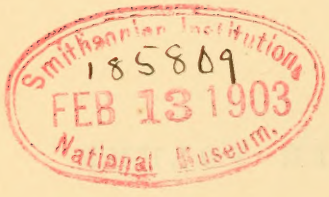

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NOTE.

The Authors of the several Papers contained in this Volume are themselves accountable for all the statements and reasonings which they have offered. In these particulars the Society must not be considered as in any way responsible.

MEMOIRS

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OF MANCHESTER.

I. *The Action of Sulphuric Acid on Diallyl.* By William Robert Jekyll, Dalton Chemical Scholar in Owens College. Communicated by Professor H. E. Roscoe, F.R.S.

Read October 18th, 1870.

Diallyl was first prepared by Berthelot and Luca in 1856. They found that it dissolves in concentrated sulphuric acid, with the evolution of much heat, and that after some hours an oil separates, which appears to be a modified hydrocarbon.

In 1866 Schorlemmer published a paper on a new series of hydrocarbons derived from coal-tar, having the formula $(C_nH_{2n-2})_2$ (Roy. Soc. Proc. xv. 132). He there says, "As these hydrocarbons were obtained by the action of sulphuric acid on coal-tar oils boiling below 120°, and as they differ by C_2H_4, it appears to me almost certain that they are polymers of the hydrocarbons of the acetylene series C_nH_{2n-2}, formed in the same way as diamylene is formed by treating amylene with sulphuric acid. In order to test this theory, I have made some experiments with the two isomers C_6H_{10}, viz. diallyl and hexoylene. By

acting with sulphuric acid on these compounds, I obtained, besides large quantities of tarry matter, polymeric modifications boiling above 200°, having a smell similar to the hydrocarbons described above, giving also similar nitro-compounds; but the quantities which I got were not large enough for a more exact examination."

In order to throw light upon this point, I undertook, at the request of Mr. Schorlemmer, to investigate the action of sulphuric acid on diallyl.

The diallyl used was obtained by the action of sodium upon allyl iodide, and boiled at 59°. Since concentrated sulphuric acid acts with great violence upon diallyl, the latter was diluted with about an equal bulk of pure paraffins, boiling at from 55° to 56°. To this mixture sulphuric acid was gradually added in small quantities, the bottle being frequently shaken. The action was attended with the evolution of much heat, so that it became necessary to keep the mixture cool by surrounding the bottle with large quantities of water. Sulphuric acid was added at intervals through the space of about six hours, and allowed to remain in contact with the diallyl during the night. Next morning the liquid was found to have separated into two layers, of which the upper one was pure paraffins, the whole of the diallyl having been taken up by the acid. The heavier and acid portion was diluted with water, when a dark-coloured oil, lighter than water, separated out, after which the whole was distilled from a large flask. The distillate consisted of a light oil, which came over below 100°, mixed with a little water. In order to free the oil from traces of undissolved paraffins and other impurities, it was redissolved in sulphuric acid, from which it was again separated by dilution and distillation. The oil was dried by means of calcium chloride, and heated for some hours over potassium, which does not act upon the pure compound. It then boiled constantly at 93° C.; and,

according to the following analyses, its composition is expressed by the formula $C_6H_{12}O$:—

(1) 0·3313 substance gave 0·869 carbonic acid, and 0·3620 water.
(2) 0·2758 „ „ 0·726 „ „ 0·2882 „

	Found. (1).	(2).	Calculated for $C_6H_{12}O$.
C	71·53	71·79	72·00
H	12·13	11·60	12·00
O	16·00	16·00	16·00
	99·66	99·39	100·00

This substance is readily soluble in concentrated sulphuric and fuming hydriodic acids, and slightly so in water. It is unacted upon by either caustic potash or potassium, and possesses a strong etherial odour like that of peppermint. In presence of potassium bichromate and sulphuric acid, this substance yields a blue colour, similar to that produced by perchromic acid and common ether.

This compound has already been obtained by Wurtz (Ann. Chem. Phys. (4) iii. 174) by treating di-iodhydrate of diallyl with silver oxide. He calls it diallyl monohydrate, but says, further on, that this body comports itself as an oxide or anhydride (ether), corresponding to dihydrate of diallyl $\left.\begin{matrix} C_6H_{12} \\ H_2 \end{matrix}\right\} O_2$, standing to the latter in the same relation as hexylene oxide to hexylene glycol; and he therefore proposes to call this compound *hexylene pseudoxide*. As I have shown that it is not acted upon by potassium, this view of Wurtz's is correct; it cannot be a hydrate; and I therefore decide to adopt Wurtz's name and to call it pseudoxide of hexylene.

It is of interest that while the action of sulphuric acid on diallyl (C_6H_{10}) produces $C_6H_{10} . H_2O$, the action of sulphuric acid on valerylene (C_5H_8) produces $(C_5H_8)_2H_2O$. (Reboul, 'Comptes Rendus,' vol. lxiv. p. 419.) This renders it important that this reaction should be investigated in

the case of hexylene, which is the true homologue of valerylene.

Oxidation of Hexylene Pseudoxide.—An oxidizing solution of the following strength was used :—

15 grams potassium bichromate.
20 grams sulphuric acid.
100 cub. cent. water.

50 cub. cent. of this solution were added to about 5 grams of the oil in each of two sealed tubes, which were exposed to a temperature of 100° for about four hours. On opening the tubes a gas was evolved, which, on passing it into lime-water, was seen to be carbonic acid. The contents of the tubes were distilled from a retort, the distillate neutralized with sodium carbonate, and evaporated on a water-bath nearly to dryness. The sodium salts were then washed into a retort with a slight excess of sulphuric acid, the distillate was neutralized with silver carbonate and filtered, on which crystals of pure silver acetate separated out, which, after drying, were analyzed with the following results :—

0·331 salt gave 0·2132 silver.

Found.	Calculated for silver acetate.
64·41 per cent. silver.	64·67.

On evaporating the mother liquor a second crop of crystals was obtained, which also consisted of pure silver acetate.

0·4497 salt gave 0·2903 silver.

Found.	Calculated for silver acetate.
64·55 per cent. silver.	64·67.

A second experiment with an oxidizing solution of the same strength yielded precisely similar results, viz. carbonic and acetic acids as the products of oxidation.

(a) 0·2365 salt gave 0·1528 silver.
(b) 0·3020 salt (from mother liquor) gave 0·1944 silver.

Found.

(a).	(b).
64·61	64·37 per cent. silver.

Nascent hydrogen evoked from sodium amalgam appears to be without action upon hexylene pseudoxide. To 100 grams of sodium amalgam, containing 5 per cent. of sodium, 15 grams of $C_6H_{12}O$, together with water, were added, the mixture being frequently shaken during the course of some hours. Next morning an additional 100 grams of sodium amalgam, containing the same percentage of sodium, were added. The pseudoxide was then allowed to remain in contact with the amalgam for two days, after which the mercury was separated from the solution of caustic, and a lighter layer, and heated in a retort; but no liquid was found to be adhering to it. The solution of soda and the lighter layer were heated in a retort, when the whole of the latter came over. The distillate, on being dried by means of calcium chloride, had the peculiar odour of hexylene pseudoxide, and boiled constantly at 93°. The oil was likewise added to sodium amalgam containing 5 per cent. of sodium, in presence of dilute hydrochloric acid; but in this case also the pseudoxide experienced no change.

Action of Hydriodic Acid on Hexylene Pseudoxide.— The action between fuming hydriodic acid and hexylene pseudoxide sets in even in the cold. A few grams of the substance were heated at 100° with an excess of fuming hydriodic acid in sealed tubes for about four hours. A red heavy liquid formed at the bottom of the tube, the contents of which were distilled from a retort, in presence of a little phosphorus. The heavy oil of the distillate was separated from the water, dried over calcium chloride, and distilled under a partial vacuum. On distillation much decomposition ensued, with the formation of hydriodic acid, a little free iodine, and with the separation of tarry matter. The distillate was a second time distilled under a partial vacuum, upon which less decomposition took place, and the greater portion came over at about 130°. It was then

dried over caustic potash, and distilled at the ordinary pressure of the atmosphere, when it was found to boil at 165°–167°, which is the boiling-point of the β hexylic iodide of Wanklyn. The following iodine determinations, made by means of an alcoholic solution of nitrate of silver further show the substance to be hexyl iodide.

(a) 0·2055 substance gave 0·2162 silver iodide, and 0·0045 silver.
(b) 0·6084 ,, ,, 0·6615 ,, ,, 0·0048 ,,

Found.		Calculated for
(a)	(b)	$C_6H_{13}I$.
59·43	59·68 per cent. iodine.	59·90 per cent. iodine.

Hexyl iodide was also readily formed by passing hydriodic acid into the pseudoxide, contained in a flask surrounded by ice. The acid was readily absorbed, with the formation of water; and a heavy dark red liquid, but no free iodine, separated out. The heavy oil evidently contained, in addition to the hexyl iodide, a body which underwent complete decomposition, even *in vacuo*. The results which I have stated as obtained by the action of hydriodic acid on hexylene pseudoxide differ somewhat from those obtained by Wurtz (Ann. de Chem. (4), iii.), inasmuch as he obtained a substance which contained 64·4 per cent. of iodine, and which he considered to be a mixture of $C_6H_{10}.H_2I_2$, and $(C_6H_{10}H_2)O.HI$. The following is perhaps the reaction which takes place when fuming hydriodic acid acts upon hexylene pseudoxide:—

$$3C_6H_{12}O+4HI=2C_6H_{12}.HI+C_6H_{10}\left\{\begin{matrix}(OH)_2\\I_2\end{matrix}\right.+H_2O,$$

$C_6H_{10}\left\{\begin{matrix}(OH)_2\\I_2\end{matrix}\right.$ being the body which undergoes decomposition on distillation. If this should be the reaction, the discrepancy between Wurtz's results and mine may be explained; for he did not distil the product which yielded 64·4 per cent. of iodine, which very nearly resembles the

percentage of iodine (viz. 63·98 per cent.) contained in the mixture $2C_6H_{12} . HI + C_6H_{10} \left\{ \begin{matrix} (OH)_2 \\ I_2 \end{matrix} \right.$.

Hydride of Hexyl.—Hexyl iodide obtained from pseudoxide of hexyline was, according to Schorlemmer's method for the conversion of iodides into hydrides, acted upon by granulated zinc in presence of hydrochloric acid for about twenty-four hours in a flask attached to an upward condenser. The liquid furnished on distillation an oil which was but slightly acted upon by bromine, thus indicating that but little of the olefines had been formed. After a second distillation, the oil was purified by agitating with, and distilling from, sulphuric acid alone, and then by treating it with a mixture of nitric and sulphuric acids. The distillate, after neutralizing with dilute caustic soda, and drying over calcium chloride, boiled constantly at 68°–69°. The analyses below show this substance to be hexyl hydride :—

(a) 0·3575 substance gave 1·0945 carbonic acid, and 0·5245 water.
(b) 0·3942 „ „ 1·2048 „ „ „ 0·5826 „

	Found.		Calculated for
	(a)	(b)	C_6H_{14}.
C	83·49	83·35	83·72
H.....	16·30	16·42	16·28
	99·79	99·77	100·00

Oxidation of the Hexyl Iodide, derived from Pseudoxide of Hexyline.—A few grams of the iodide were heated in a flask attached to an upward condenser for four hours, with an oxidizing solution of the following strength :—

20 grams of potassium bichromate.
30 grams of sulphuric acid.
60 cub. cent. of water.

During the operation much carbonic acid was evolved. At the end of four hours, the same amount of an oxidizing solution of the same strength was added to the contents of the flask, which were heated together for the space of

three hours. The mixture was distilled from a retort, the distillate neutralized with sodium carbonate, and evaporated nearly to dryness on a water-bath. The sodium salts were washed into a retort, from which the combined acid was fractionated by means of successively adding five drops of sulphuric acid, and catching the distillates separately after each addition of acid. The first four distillates were acid, and were neutralized by silver oxides. After filtration, salts separated out, which were analyzed with the following results :—

No. 1. ..... 0·2018 salt gave 0·1198 silver.
No. 2. (a) ... 0·1025 „ 0·0683 „
(b) ... 0·1095 (from mother liquor) gave 0·0715 silver.
No. 3. ..... 0·1960 salt gave 0·1257 silver.
No. 4. (a) ... 0·3204 „ 0·2072 „
(b) ... 0·1952 (from mother liquor) gave 0·1255 silver.

	Found.	Calculated for silver acetate.
No. 1.	59·36	64·67
„ 2. (a) ...	66·63	„
(b) ...	65·29	„
„ 3.	64·13	„
„ 4. (a) ...	64·66	„
(b) ...	64·29	„

The non-agreement of the analyses of the salts derived from distillates 1 and 2 with the calculated results, was explained by the impure nature of the salts, which were non-crystalline, and which I could not succeed in rendering pure, though they were several times redissolved. Since only carbonic and acetic acids, but no butyric, had been obtained from this hexyl iodide, while its boiling-point resembled that of Wanklyn's β hexylic iodide (for the boiling-point of the iodide prepared from $C_6H_{12}O$ in no case reached 170°), and since the alcohol prepared from the β iodide yields butyric acid among its oxidation products (Journ. Chem. Soc. New Ser. i. 221), it became necessary to repeat the experiment with a weaker oxidizing solution; therefore a few grains of the iodide were heated

for about five hours with the following oxidizing solution in a flask, in connexion with an upward condenser :—

20 grams potassium bichromate.
30 „ sulphuric acid.
120 cub. cent. water.

In this case less carbonic acid was evolved than in the former one. The contents of the flask were distilled from a retort, the distillate being neutralized with sodium carbonate. The neutral solution was distilled with an excess of sulphuric acid. The distillate, containing in a free state the acids of oxidation, was slowly distilled four times from a retort, the residues being preserved in each case, and the final distillate rendered just neutral by sodium carbonate. About two thirds were slowly distilled off the collected residue, the final residue from which was neutralized by silver oxide filtered, and the silver salt analyzed. It is evident, if the two acids, acetic and butyric, had been formed, that almost the whole of the acetic would be in the final residue, and the butyric in the final distillate. The analyses below show that the salt obtained from the final residue consisted of pure silver acetate :—

(a) 0·1447 salts gave 0·094 silver.
(b) 0·3362 salts (from mother liquor) gave 0·2183 silver.

Found		Calculated for
(a)	(b)	$C_2H_3O_2$ Ag.
64·96	64·93 per cent. silver.	64·67 per cent. silver.

The two thirds which were distilled off from the collected residues were added to the neutral sodium salt of the final distillate, and the whole distilled from a retort; the resulting distillate, however, did not furnish sufficient acid for analysis. Five drops of sulphuric acid, together with water, were added to the contents of the retort, and distillation was recommenced. From the distillate a silver salt was obtained, which the following analyses show to be also silver acetate :—

(a) 0·2452 salt gave 0·1595 silver.
(b) 0·2745 salt (from mother liquor) gave 0·1781 silver.

Found.	
(a)	(b)
65·05	64·88 per cent. silver.

On the further addition of sulphuric acid to the contents of the retort, no acid distillate was obtained. It is thus seen that in this second experiment, where an oxidizing solution was employed of only one-half the strength used in the former one, precisely the same results are obtained, viz. carbonic and acetic acids—which, it may be remarked, are the same oxidation products as were yielded by the oxidation of the pseudoxide.

The diluted sulphuric acid, which had been used for acting upon diallyl, was neutralized with barium carbonate, filtered and evaporated to dryness on a water-bath; but a sulpho-organic barium salt was not obtained.

Polymers of Diallyl.—After distilling off the pseudoxide of hexylene from the diluted acid, a layer of hydrocarbons remained on the surface of the liquid, from which they were separated by means of a stop-funnel. The hydrocarbons were dried over calcium chloride, and found to to boil at between 200° and 300°. After several distillations over metallic sodium, they were separated into three portions, boiling at from 205°–215°, 240°–245°, and 275°–285°. These hydrocarbons invariably left a slight residue on distillation. The analyses below show that they have an empirical formula of C_6H_{10}.

(1) Boiling-point 205°–215°.

(a) 0·2168 substance gave 0·6941 carbonic acid, and 0·2445 water.
(b) 0·2340 „ „ 0·7498 „ „ 0·2593 „

	Found.		Calculated for
	(a)	(b)	C_6H_{10}.
C	87·31	87·38	87·8
H	12·52	12·31	12·2
	99·83	99·69	100·0

(2) Boiling-point 240°–245°.

(a) 0·2187 substance gave 0·7001 carbonic acid, and 0·2446 water.
(b) 0·1545 ,, ,, 0·4946 ,, ,, 0·1713 ,,

	Found. (a)	(b)	Calculated for C_6H_{10}.
C	87·30	87·30	87·8
H	12·42	12·31	12·2
	99·72	99·61	100·0

(3) Boiling-point 275°–285°.

0·1760 substance gave 0·5612 carbonic acid, and 0·2029 water.

	Found.	Calculated for C_6H_{10}.
C	86·96	87·8
H	12·81	12·2
	99·77	100·0

No. 3 attacked sodium slightly, although it had been distilled over it several times; hence it is probable that its non-agreement with the calculated result was owing to admixture with an oxygen compound. From the above analyses and boiling-points it is probable that at least two, and possibly three, polymers of diallyl are formed by the the action of sulphuric acid upon it. I had not, however, a sufficient quantity of the hydrocarbons to obtain satisfactory vapour-density determinations, which would at once have settled the point. It is nevertheless very probable that No. 1 consists of two molecules of diallyl condensed into one, and that it has the formula $C_{12}H_{20}$; for Schorlemmer (Proc. Roy. Soc. xv. 133), by the action of sulphuric acid on hydrocarbons boiling below 120° from cannel oil, obtained one which boiled at 210°, and the vapour-density of which showed that its formula was $C_{12}H_{20}$.

Action of Bromine.—Bromine was allowed to drop very slowly into the hydrocarbon boiling at 240°–245°, which was contained in a test-tube kept cool by being surrounded

with water. Combination took place with a hissing noise, and with the evolution of a considerable amount of hydrobromic acid. After the hydrocarbon had been treated with an excess of bromine for several hours, the excess was removed by means of a dilute solution of caustic soda. The bromine product having been separated from the caustic soda, it was dried over calcium chloride and dissolved in ether, which was got rid of by keeping the solution in a water-bath for about six hours, at the end of which time the bromine product was left in the form of a brown solid that became viscid at 100°. This crude product, without further purification, was analyzed, with the following results:—

0·5172 substance gave 0·5546 silver bromide, and 0·0048 silver.

Found.	Calculated for $C_{12}H_{20}Br_2$.	Calculated for $C_{18}H_{30}Br_2$.
46·30 per cent. bromine.	49·38	39·40 per cent. bromine.

Hence it appears probable that the oil boiling at 240°–245° is a mixture of the hydrocarbons $(C_6H_{10})_2$ and $(C_6H_{10})_3$, while the formula of the one boiling at 205°–215° is $(C_6H_{10})_2$, and of the one boiling at 275°–285° is $(C_6H_{10})_3$.

In conclusion, I have much pleasure in tendering my thanks to Dr. Roscoe and Mr. Schorlemmer for their kindness and attention to me throughout the whole course of this research.

II. *On Isodinaphthyl.* By Watson Smith, F.C.S. Communicated by Professor H. E. Roscoe, Ph.D., F.R.S.

Read November 29th, 1870.

Instead of at once dealing with the main subject of this paper, I think a brief view of certain of the results of researches of several eminent chemists, directly bearing on the matter in hand, would be at once interesting and desirable.

The peculiar hydrocarbon diphenyl was first obtained by Fittig* by treating monobrombenzol with sodium :

$$2C_6H_5Br + 2Na = 2NaBr + \left.\begin{matrix} C_6H_5 \\ C_6H_5 \end{matrix}\right\}.$$

Now Berthelot†, in the year 1866, by passing benzol vapour through a porcelain tube heated to bright redness, obtained diphenyl as the chief product of decomposition. Berthelot distinctly says, "The body so obtained is a beautifully crystalline substance, which is identical with Fittig's diphenyl, possessing exactly the melting- and boiling-points given by Fittig, and agreeing generally with that body." A small quantity of chrysene is also said to be formed, and of a solid resinous body, of an orange colour, which is but slightly soluble in alcohol, rendering the same fluorescent. The formula

$$2C_6H_6 = \overset{\text{Diphenyl.}}{\left.\begin{matrix} C_6H_5 \\ C_6H_5 \end{matrix}\right\}} + H_2$$

represents this decomposition.

I. About the commencement of the year 1867‡, Dr.

* Ann. d. Ch. und Ph. cxxi. 361, and cxxxii. 201.

† Zeitsch. f. Chemie, 1866, Dec. 14, p. 707.

‡ Ann. d. Ch. und Pharm. Band cxliv. p. 71.

F. Lossen found that dinaphthyl could be obtained from monobromnaphthalin by the action of sodium, just as Fittig had obtained diphenyl from monobrombenzol:

$$2C_{10}H_7Br + 2Na = 2NaBr + \left.\begin{matrix} C_{10}H_7 \\ C_{10}H_7 \end{matrix}\right\}.$$

At the same time he (Lossen) found there was also a small quantity of a polymer formed, which he obtained as a chocolate-brown powder, scarcely at all soluble in alcohol, but more so in ether. It was easily soluble in carbonic disulphide.

II. Dr. Lossen also found that he could obtain dinaphthyl by the oxidizing action, upon naphthalin, of a mixture of manganese peroxide and sulphuric acid:

$$2\left.\begin{matrix} C_{10}H_7 \\ H \end{matrix}\right\} + O = \left.\begin{matrix} C_{10}H_7 \\ C_{10}H_7 \end{matrix}\right\} + H_2O.$$

This mixture he found acted energetically upon naphthalin, there being liberated, with strong effervescence, much carbonic dioxide. When the action was completed, the contents of the retort in which the process was carried on were diluted with water, manganese sulphate and phthalic acid were dissolved out, and unaltered manganese peroxide and a resinous body left behind. The undissolved portion was boiled with absolute alcohol; and the impure body crystallized out from the filtered solution was repeatedly recrystallized from alcohol. The dinaphthyl was thus obtained in pearly shining scales of a very light yellow colour. Lossen found that it was only possible to obtain the body quite colourless by sublimation.

Thus obtained, dinaphthyl was found to be much more easily soluble in ether than in alcohol, crystallizing therefrom in moss-like clusters. By slowly cooling a solution of this body in a mixture of ether and alcohol, it was obtained crystallized in well-formed regular octahedra. It is very soluble in carbonic disulphide. Benzol dissolves it to

much the same extent as alcohol does; and it crystallizes from the solution of that body in the same form as it does from alcohol. Dinaphthyl melts at 154° C. Its boiling-point lies above that of mercury. Robert Otto* and Gustav Möries repeated Lossen's experiments with precisely the same results; but they tried, in order to get dinaphthyl formed more easily, and in larger quantity, to form it by first obtaining the mercury compound mercurio-dinaphthyl $\left.\begin{matrix}C_{10}H_7\\C_{10}H_7\end{matrix}\right\}Hg''$, by heating monobromnaphthalin with sodium amalgam (Na_2Hg''), and raising this to a red heat with soda lime in a porcelain tube. Mercury and a yellow crystalline mass were obtained, the greatest portion of the latter consisting of naphthalin. This yellow mass, on boiling with alcohol, yielded on cooling the rhombic tables of naphthalin abundantly, the mother liquors exhibiting strong green fluorescence by reflected light, and furnishiug on evaporation a yellow mass, which, after suitable purification, deposited from alcohol small canary-yellow shining leaf-like crystals, which melted at 133° C. This body was readily soluble in alcohol, ether, and benzol. The solution of the pure hydrocarbon was no longer fluorescent.

They made a combustion, which did not seem to be satisfactory, as they only had 0·066 grm. of substance to work with, and of this they did not feel sure of the perfect purity. The percentage composition they obtained almost exactly corresponded with that of naphthalin. They consider the action to be chiefly

$$\left.\begin{matrix}C_{10}H_7\\C_{10}H_7\end{matrix}\right\}Hg'' + H_2O = 2C_{10}H_8 + HgO.$$

About the close of the year 1866, Berthelot† concluded his remarkable series of experiments on the action of cer-

* Ann. d. Ch. und Ph. 1868, cxlvii. p. 164.

Zeitschrift f. Ch. 1867, Jan. 31st; and Compt. Rend. lxiii. 998 and 1077.

tain hydrocarbons upon one another, when conducted through a red hot tube. In one of these experiments he passes the vapour of naphthalin with hydrogen through the tube; and as to the result he says, "Almost no action at all takes place, the naphthalin remaining almost quite unaltered, and only a little benzol and acetylene are obtained."

$$\underset{\text{Naphthalin.}}{C_6H_4[C_2H_2(C_2H_2)]} + H_2 = \underset{\text{Benzol.}}{C_6H_6} + \underset{\text{Acetylene.}}{2C_2H_2}.$$

About nine months ago I undertook, at the suggestion of Mr. John Barrow, in the laboratory of whose works I was then engaged, the investigation of the action of heat upon naphthalin, with the object of discovering whether by this means it is possible to obtain anthracene, according the equation

$$7(C_{10}\ H_8) = 5C_{14}H_{10} + 6H.$$

This reaction Mr. Barrow supposed might take place on passing the naphthalin vapours through a red-hot tube. I accordingly tried a number of preliminary experiments in this direction, of which the results were as follows :—

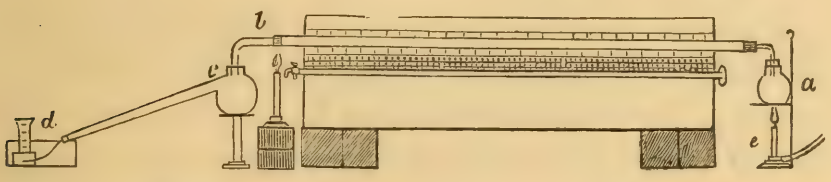
1st. The odour and colour of the naphthalin which came through the tube were perceptibly altered; the colour became a deeper brown in proportion as the temperature of the tube was higher. At an incipient white heat a considerable quantity of black smoke came over with the distillate.

2ndly. A gaseous product was obtained proportionately larger in amount as the temperatnre employed was higher.

3rdly. On distilling the brown mass of naphthalin coming through the tube, there was invariably left in the retort a minute quantity whose boiling-point was above 350° Cent.

I now prepared some pure naphthalin, by treating a quantity of the crude stuff, whilst fused, with warm concen-

trated sulphuric acid (about 5 p. c. by volume); and, after separating, it was again shaken with hot caustic soda solution of sp. gr. = 1·18. The naphthalin thus treated was now purified by fractional distillation, the portion boiling between 210° C. and 212° C. being kept. After another distillation, the perfectly white product obtained was taken for experiment. Into a small flask (*a*) 152·6 grms.



a. Small flask with naphthalin.
b. ½-inch iron tube, filled with small fragments of charcoal.
c. Retort, to collect distillate.
d. Gas-jar, to collect and measure gas evolved.

of the pure naphthalin were introduced, and the flask was connected with an iron tube (½ inch diameter) filled with small fragments of charcoal, to afford a large heated surface. That charcoal had no chemical influence beyond that of affording a large heated surface, was proved by the fact that the same results were obtained when fragments of pumice stone, soda-lime, brick, or the empty tube itself was employed. With the other end of the iron tube was connected a bent glass tube (*b*) (¼ inch diameter), fitting air-tight into a retort (*c*) so deep that the end thereof should be below the aperture of the retort-stem. With the extremity of the retort-stem was connected another bent glass tube, dipping into the pneumatic trough, over which was a jar of 340 cub. cent. capacity, to collect the gaseous products evolved. The iron tube was placed in a "Griffin's gas-furnace," where it was raised to a bright red heat, in some parts approaching to whiteness. When the iron tube was red hot, the naphthalin was distilled through cautiously. As soon as it made its appearance in the tube

(*b*), the latter was heated up somewhat strongly, so as not to allow the solidification of the naphthalin. The distillate, when it commenced to run down the bend to the cool part of the tube in the retort, was worked over for a minute or two as briskly as possible; or it would certainly have stopped up that cool part of the tube which dipped below the stopper of the retort, causing a dangerous explosion from the little flask (*a*).

When the naphthalin began to run fluid into the retort, the distillation was kept up briskly for a time, all solidified particles being speedily remelted by the hot stream, and carried before it into the retort. All danger of stopping up then ceased, as the whole apparatus was too hot to permit solidification in any part thereof.

The substance distilling over was of a deep brown-red colour, and had acquired a peculiar odour, in addition to that usual in naphthalin. The gaseous product obtained was just sufficient in the first trial to fill the gas-jar (340 cub. cent.). The distillation was carried on till the flask was just dry, when, on removing the flame of the lamp, there was always a back rush of vapour, accompanied by the dropping back of a dark-looking fluid, amounting in this (the first) distillation to about 1 cub. cent.

This small quantity of dark-looking substance which had dropped back out of the hot tube, was poured out into a small capsule, and carefully preserved, as the first contribution towards a quantity of the crude substance.

What distilled over into the retort (*c*), of reddish-brown colour, was now fused, and poured back again into the little distillation-flask (*a*), from which it was again distilled through the red-hot tube as before, in the first experiment. It not only yielded a little freshly formed substance of high boiling-point, but also left behind a little which had been formed, and carried over amongst the bulk of the naphthalin, in the first distillation. Thus

a somewhat larger yield was obtained in the second and subsequent distillations than in the first. These distillations were repeated seven times, to accumulate crude material. The temperatures at which the tube was maintained were not uniform in each experiment; and the volume of gas evolved was found to vary accordingly. Thus in the second distillation, the heat was only just perceptibly red, and very little gas was obtained (about 5 cub. cent. or so).

Quantities of Gas liberated during each experiment, the heat employed varying in intensity.

Distillation.	Intensity of heat of tube.	Cub. cent. of gas evolved.
1st.	Bright red heat.	300
2nd.	Only just red.	Very little, about 5 cb.c.
3rd.	Bright red.	460
4th.	Do.	300
5th.	Do.	500
6th.	Do.	403
7th.	Do.	300
8th.	Do.	225
		Total 2493
		Average temperature=15° C.

The above measurements of gas evolved were not made with any great accuracy, but are just fair approximations. This gas was tested, and yielded the following results :—

1st. It gave no milkiness with lime-water.

2ndly. It burnt, on ignition, with a lambent bluish flame, tinged at times with a glimmering of the white carbon-flame, and on shaking with lime-water gave therewith a very slight milkiness; but almost always there was not the slightest glimmer of the carbon-flame, and not the slightest cloud with lime-water after burning.

3rdly. The gas, mixed with $\frac{1}{2}$ its volume of oxygen and inflamed, exploded loudly.

From these evidences we gather that the gas is hydrogen; and the glimmering at times of the carbon-flame was pro-

bably due to the presence of a little naphthalin vapour mechanically carried over, though certainly a trace of acetylene might be present at times, and would then account for it.

If a calculation be made from the volume of gas evolved, it will be found that this quantity is too great to stand good for the equation

$$7C_{10}H_8 = 5C_{14}H_{10} + 6H.$$

Weight of H actually evolved . . . . =0·2107 grm.
„ „ required for the above reaction=0·1861 „

The above is sufficient to show that, as the amount of hydrogen evolved was too great to stand good for the above equation, the body formed from the decomposition (seeing that there was no appreciable separation of carbon) must be richer in carbon and poorer in hydrogen than anthracene is. This point will be again referred to.

Now the crude substance which had been accumulated amounted to 26·3 grms.—that is, 17·23 per cent. on the naphthalin used. These 26·3 grams of crude substance were placed in a small retort with thermometer, and submitted to fractional distillation. A very small quantity of naphthalin came over first, below 220° C.; and then the temperature very rapidly rose to considerably above 360° C., when most of the body distilled over, leaving a minute quantity of pitch in the retort. In distilling, almost all the substance solidified in the neck of the retort, which had to be kept very hot towards the end of the operation, to get the substance to run down fused into the receiver. At this stage the distilled product closely resembled anthracene in a similar stage of purification, but did not possess the peculiar odour which characterizes that body in the crude state, being almost entirely free from odour.

To purify further, the solid hard substance was crushed in a mortar and made into a thin homogeneous paste with

light shale-spirit (or petroleum-spirit), in both of which it is much less soluble than in benzol. It was then drained on a filter, washed again several times with petroleum- or shale-spirit, drained, pressed in blotting-paper, and dried at a temperature fully sufficient to volatilize the spirit. By this washing it was freed from a small quantity of resinous matter, and from a citron-yellow-coloured substance, which coloured the spirit mother-liquors yellow by direct transmitted light, exhibiting a fine blue fluorescence by reflected light.

This fluorescence is similarly acquired when any fluids possessing any solvent action are substituted for the shale-spirit.

Finally the dried substance was sublimed at as low a temperature as possible. The sublimate consisted of a faintly yellowish white inodorous powder, precisely resembling commercial anthracene sublimed under like conditions, but not possessing the slightest odour. The sublimate may also be obtained in delicate little plates, by very careful and slow sublimation. When possessing the faintest yellow tint, this powder imparts a delicate blue fluorescence to any solvent used.

In another preparation of a further quantity of crude substance, a large iron tube, about 4 or 5 feet long, and about $3\frac{1}{2}$ inches diameter was used, being placed in a charcoal fire. The heat was almost to whiteness. In this case there was a connexion with a pneumatic trough, the open end of the tube dipping into an iron pan, acting as receiver. Immense volumes of black smoke were liberated at first*; however, on moderating the heat somewhat, whilst the dense black smoke still poured out, a quantity of undecomposed naphthalin and other matters came over, dropping into the receiver. The black, almost

* Probably caused by the admittance of a small amount of air accidentally.

solid mass in the receiver, after being put back into the one-gallon iron still employed, and redistilled several times, at length left a residue, which, on fractional distillation, proved to contain some undecomposed naphthalin, this passing over below 220° C. A very appreciable residue however remained, which was distilled over, the temperature rising above 350° C. In this case, however, the solid hard substance obtained had a deep yellow tint, almost inclining to orange; and on triturating in a mortar with benzol (cold), the latter dissolved out a sticky resinous body, which coloured it a fine deep red. After washing once or twice, the crude body remaining was quite freed from this sticky substance, and remained a sulphur-yellow powder. This yellow colour was very much more intense than in the case of the experiments on the minor scale. By recrystallization from benzol and sublimation, it was obtained colourless.

A small quantity of this crude orange-yellow body, treated with cold concentrated sulphuric acid, yielded a a very fine purple solution, better developed on warming. On heating more strongly, the colour changed to an indigo tint, and then to a dull green. As this reaction is mentioned by C. Liebermann as characteristic of chrysene, I venture to suggest that probably chrysene was present in this yellow crude substance.

With a benzol solution of picric acid in excess, no red compound was formed with this body. It was found to be much more insoluble in alcohol and shale-spirit than anthracene is; from these solutions it separates on cooling in larger or smaller rhomboidal plates, according to whether the solutions are more slowly or rapidly cooled. It is almost insoluble in ether, but easily soluble in oil of turpentine, from which it crystallizes in beautiful lance-shaped crystals, congregating in clusters. It is, like anthracene, very soluble in carbonic disulphide, even in the cold. The tho-

roughly purified substance imparts no longer any fluorescence to solvents.

Action of Solvents, in the Order of Solubility.

Lossen's dinaphthyl: solubility &c.	Isodinaphthyl.		
	Solvents.	States of Solubility.	Crystalline form.
The reverse; more soluble in ether than in alcohol.	Proof spirit.	Scarcely at all, even on boiling with excess.	On cooling, a few small plates separate out.
	Ether.	Only very slightly.	Beautiful rhomboidal plates, which, on drying carefully, overlap and laminate, and have a beautiful silky appearance. They possess a very faint, delicate greenish colour.
	Abs. alcohol.	Soluble on boiling.	
From alcohol separates in moss-like clusters.	Methylated spirit.	More so.	
In octahedra from ether-alcohol mixture.	Wood spirit.		
	Light petroleum-spirit, sp.gr. ·710.	Still more soluble than in the above.	Rhomboidal plates.
	Light shale-spirit, sp. gr. =·737.	Rather more than in petr. spirit.	
	Carbon tetrachloride.	Slightly less soluble than in benzol.	
About the same as in alcohol.	Benzol.	Sparingly in the cold, pretty freely in the hot fluid.	Ditto.
Easily soluble.	Carbonic disulphide.	Easily soluble, even in the cold.	
	Oil of turpentine.	Very easily soluble.	Delicate white lance-shaped crystals, in clusters.
Melting-point =154° C.	Melting-point =204° C.		
Boiling-point = above 350° C.	Boiling-point =much above 360° C.		

The fusing-point is from 200° to 204° C., and the boiling-point considerably over 360° C., certainly higher than that of anthracene. The subliming-point of this body lies considerably below its boiling-point, like that of anthracene.

A portion of the pure substance was treated with two

parts of potassium bichromate and sulphuric acid, when oxidation took place, as energetically as in the case of anthracene under like treatment. No crystals bearing any resemblance to those of anthrachinon could be obtained on treating with benzol. From the product which was obtained, no substance similar to alizarin, nor in fact any colouring-matter, could be obtained.

Cold sulphuric acid is without action upon this body; but when heated the acid slowly dissolves it, when, if it be quite pure, a very faint purplish-tinted solution is formed; if not quite pure, but containing a trace of the yellow-coloured body mentioned before, a violet or purple solution is formed, becoming green and then reddish-brown on further warming. (*Vide* page 22.)

Hot nitric acid oxidizes it, with liberation of nitrous fumes, forming heavy oily drops of a nitro-compound of a red colour; on further boiling, with a larger excess of acid, this compound is slowly dissolved. On addition of water, a light yellow floccular precipitate is thrown down abundantly.

Placed in a glass tube and chlorine passed over it in the cold, no action takes place; and even on warming to a considerable extent, no change appears to occur. Treated with bromine in the cold, combination takes place with very energetic action.

After many repeated experiments on the subject, I have come to the conclusion that it is impossible to distil pure naphthalin to dryness in any quantity without this body being formed in minute quantity. This may be observed by dipping the thermometer down nearly to the bottom of the retort, when the last minute quantity is being vapourized; the thermometer lowered into this vapour, will be observed speedily to indicate a high temperature, the mercury rapidly rising towards 350° C. If this does not take place on the first distillation,—if the naphthalin,

all of which has passed over, be again introduced into the retort, and the same plan again proceeded with, this observation may be made successfully, as a small quantity of high boiling substance carried over in the first distillation will be increased by a further accumulation in the second, and an appreciable quantity will be left behind.

A quantity of the pure substance crystallized from absolute alcohol was taken for analysis. The following results were obtained :—

I. 0·1240 grm. of substance gave 0·4307 gram CO_2 } 0·0624 „ H_2O }

II. 0·1237 „ „ „ 0·4284 „ CO_2 } 0·0626 „ H_2O }

	I.	II.	Calculated for $\frac{C_{10}H_7}{C_{10}H_7}$ }
Carbon	94·72	94·46	94·49
Hydrogen	5·59	5·62	5·51
	100·31	100·08	100·00

A calculation may also be made from the weight of hydrogen given off in the reaction in the hot tube; and I think it is well worthy of notice, as it was this observation which first led me to think the reaction must be as follows, viz. :— $2C_{10}H_8 = \frac{C_{10}H_7}{C_{10}H_7} \} + 2H.$

Weight of naphthalin converted =26·3 grms. (about).
Volume of hydrogen at 0° C. evolved =2363 cub. cents.

Then $\frac{2363}{11200}$ =0·2109 grm. hydrogen actually obtained.

256 2
$2C_{10}H_8$ lose H_2 ∴ 26·3 lose 0·2055 „ „ calculated.

Difference ..... 0·0054

For the formula
$7C_{10}H_8 = 5C_{14}H_{10} + 6H$, 0·1861 „ „ should be liberated.

Conclusions.

I find from the foregoing facts :—

1st. That whereas Berthelot found that diphenyl

could be obtained by the action of a high temperature upon benzol, identical with that obtained by Fittig from brombenzol, the analogy does not appear to hold good with dinaphthyl formed from naphthalin.

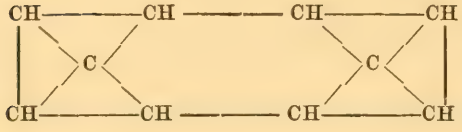
2ndly. That a substance having a higher melting-point, and differing in many respects from Lossen's dinaphthyl, is formed.

3dly. That this body is the chief product of the action of heat upon naphthalin, whether a low temperature, just sufficient to induce decomposition, or a temperature close upon a white heat be employed.

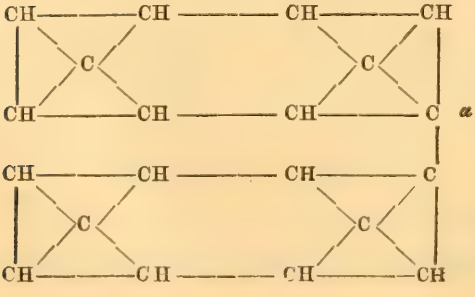
Finally. Observing the percentage composition of this body, and the volume of hydrogen approximately found to be evolved in the decomposition, also that the boiling-points of dinaphthyl and this body are by no means very widely different, possibly not far apart, I propose to regard it as an isomer of dinaphthyl, and to name it accordingly isodinaphthyl,

$$\left.\begin{matrix} C_{10}H_7 \\ C_{10}H_7 \end{matrix}\right\}.$$

If naphthalin be graphically represented as

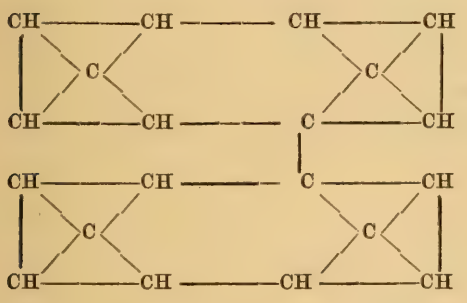


Dr. Lossen's dinaphthyl would appear to be



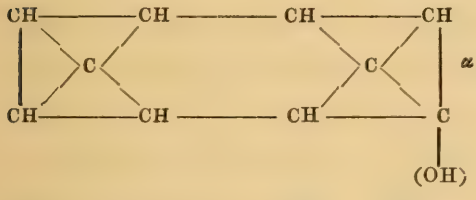
α Dinaphthyl.
Melting-point = 154° C.

whilst the isodinaphthyl obtained by myself would stand

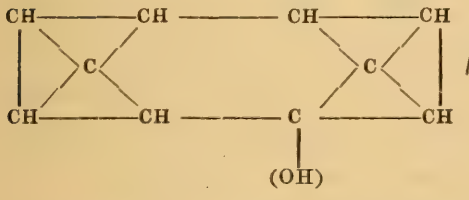


β Dinaphthyl.
Melting-point = 204° C.

Analogously, the *α* and *β* naphthols investigated by Wichelhaus, and also by Schaeffer (Ann. d. Chemie und Pharm. clii. 281), would appear thus :—



α Naphthol.
Melting-point = 94° C.
Boiling-point = 279° C.



β Naphthol.
Melting-point = 122° C.
Boiling-point = 287° C.

Action of Chlorine.—The effect of passing chlorine together with naphthalin vapour through a red-hot tube was also tried in such a way that the vapours should only come into contact in the hot tube. It was then found that complete decomposition took place, with large separation of carbon in dense black smoke.

I cannot conclude without heartily thanking Mr. Barrow, in the laboratory of whose works these experiments were carried out, for his kind assistance. My sincere thanks are also due to Prof. Roscoe and Mr. Schorlemmer for their kindness in placing the combustion-apparatus in Owens College laboratory at my disposal, and for their kind attention to me whilst there engaged.

III. *On the Organization of* Volkmannia Dawsoni, *an undescribed Verticillate Strobilus from the Lower Coal-measures of Lancashire.* By W. C. Williamson, F.R.S., Professor of Natural History in Owens College, Manchester.

Read February 7th, 1871.

In his "Observations on the Structure of Fossil Plants found in the Carboniferous Strata," Mr. Binney has figured three types of verticillate spikes of cryptogamic fructification, which in all probability represent three very different groups of plants. In plates 4 and 5 he has given the structure of one of these types under the name of *Calamodendron commune*. The same type has received from Mr. Carruthers the name of *Volkmannia Binnei* (Journal of Botany, Dec. 1867), whilst Prof. Schimper has designated it *Calamostachys Binneyana*. At the last Meeting of the British Association at Liverpool, I advanced my reasons for believing that this spike had no relationship with the group of Calamites; and the same view is advanced in a still more recent "Memoir on the Organization of Calamites," presented to the Royal Society. In the type referred to, as is well known, the several nodes of the central axis give off bractigerous disks and sporangiophores in alternate verticils. In his plate vi., Mr. Binney figures a spike (fig. 1), in which the sporangia are very compactly clustered in verticils, which are very closely embraced by a bractigerous disk springing from each node. I believe this figure represents the same plant as that of which I recently described the internal organization ("On a new form of Calamitean Strobilus from the Lancashire Coal-measures," Memoirs of the Literary and

Philosophical Society of Manchester, Third Series, vol. iv. p. 248); and I have not the slightest doubt that it represents a true Calamitean fructification. The reasons for this firm conviction are given in the memoir refered to.

But Mr. Binney also figures a third form (tab. vi. fig. 4), in which the spikes have a much more lax habit. The bracts fringing the bractigerous disk do not embrace the sporangia, but project from beneath them in a rigid manner, leaving the sporangia, loosely located between each verticillate bractigerous disk, conspicuously exposed to view. The same type of fructification has recently been found in carboniferous shales exposed in a railway-cutting at Huyton, near Liverpool, by the Rev. H. Higgins.

In a second, brief memoir ("On the Organs of Fructification and Foliage of *Calamodendron commune* (?)," Memoirs of the Literary and Philosophical Society of Manchester, Third Series, vol. iv. p. 218), Mr. Binney has figured a second specimen of what I believe to be the true Calamitean type of fruit.

Mr. Butterworth placed in my hands some months ago a small strobilus which he had found in the Lower Coal-measures near Oldham; and on investigating its organization I soon found that it belonged to the third of the three types to which I have directed attention, viz. that with rigid bracts and exposed sporangia. Its internal structure demonstrates that it is altogether different from either of the other two types referred to, and evidently belongs to a different plant. A fragment of the fruit, which we have retained unsliced, demonstrates its close resemblance to Mr. Binney's tab. vi. fig. 4.

The spike or strobilus now to be described consists of a central axis, sustaining successive concavo-convex disks, which divide at their peripheral margins into a considerable number of long, stiff, slightly curved bracts. Between each of these bractigerous disks is a single layer of spo-

rangia, arranged in about four irregular circles. Each sporangium appears to have its own sporangiphore, of which considerable numbers spring from the upper surface of each disk.

The Axis.—This resolves itself into a central vascular mass (*a*) lodged in what is now a clear space (*c*), but which was at one time doubtless occupied by cellular tissue, the greater part of which has disappeared, either by absorption or decay. This space is surrounded by a cellular cylinder (*b*) consisting chiefly of a dense form of prosenchyma, and of which the bractigerous disks are extensions. The form assumed by the central vascular axis (*a*) constitutes one of the most characteristic features of the organism. Its transverse section (fig. 5) may be described as a triangle with concave sides (*a* 1) and concavely truncated angles (*a* 2). It consists of a mass of vessels, which are largest in the central portions, and smallest and most dense at the periphery, especially at the angles (*a* 2). At each of the latter portions the structure becomes broader, in consequence of the projection of two lateral vascular extensions (*a* 4) than it is nearer the centre. From each of these projections a line of compressed and disorganized cellular tissue (*a* 3) extends like a chord across the arc described by the concave side of the triangle, and at *a* 5 we see a similar band stretching across the concavely truncated angle. At *a'* 3 and *a'* 5 we see clear proof that these have originally been layers of cellular tissue that have closely invested the vascular axis, but which have become detached and shortened during some of the morphological changes which the plant has undergone. Most probably this axis originally occupied the centre of the open cylindrical space *c*, but has been drawn to one side, on the disappearance of the cellular tissue surrounding it. Fig. 5 *b* represents a portion of the outer or prosenchymatous cylinder of the axis. In the longitudinal section, fig. 1,

we see that this central axis consists of vessels; but I am unable to discover in them any trace either of transverse bars or of reticulations. This, however, is in all probability due to the peculiar state of mineralization which the entire specimen exhibits. The general *outlines* are everywhere exquisitely preserved; but the minute texture, both of cells and vessels, is so altered that no reliance can be placed upon its indications. That the vessels have been thickened by internal deposits of some kind is clear enough. The greater diameter of the vascular axis between its most distant angles is ·065. That of the largest of its component vessels is about ·0037 of an inch. Of the tissues originally occupying the space (*c*) between the axis just described and the outer prosenchymatous cylinder (*b*), we have little or no indication, a few shreds of what may have been cellular tissue being the only remains of them.

In describing the prosenchymatous cylinder (*b*), we have to distinguish between its several parts. It gives off the bractigerous disks; or perhaps it may be more correctly defined as consisting of the central portion of each disk, prolonged downward to the node below, as shown in figures 1 and 2. It of course follows that its prosenchymatous walls are thinnest in a transverse section made immediately above each disk (fig. 4 *b*), and thickest at the points where the disk springs from it (fig. 4 *b'*). The prosenchymatous structure of its walls is shown at fig. 2 *b'*, where a tangential section has intersected the cells in the plane of their longer axes. The mean length of these cells is very variable, their diameter about ·0024 of an inch. The diameter of the exterior of the cylinder at its narrowest part, just above a node, is ·13. At each node the cylinder just described gives off a thick bractigerous disk (*d*), which extends upwards and outwards, at an angle of about 45°, curving slightly upwards and inwards as it ascends. It extends about the eighth of

an inch from the inner surface of the prosenchymatous cylinder, in an undivided manner; but its tendency to break up into subdivisions is seen in the crenulated outline *e*, in the sections figs. 3 and 4. This crenulation is the result of deep radiating grooves indenting the inferior surface of the disk. These deepen as they proceed outwards, and soon divide the disk into a verticil of separated bracts. In fig. 4 we see the first result of this division in the series of irregular masses (*f f*); but the true form of these divisions is masked in the part referred to, because the section has passed through them very obliquely, in fact almost in the plane of their two surfaces. The form and appearance of true sections are better seen at *f'* in the same figure. But their relative positions will be still better understood by referring to fig. 6, which represents a tangential section made vertically through three of the bractigerous disks, a little externally to the point at which that structure is divided into its peripheral fringe of elongated bracts. Sections of these bracts are seen at *f'*, whilst at the side of the cone, *f''*, these bracts extend upwards to their termination in the plane of the section. In the latter portions we see that their two epidermal surfaces appear to have been more dense and solid than their central portions. On turning to tranverse sections of these uppermost portions of the bracts, as we see them at *f''* *f''* in figures 3 and 4, we discover that they here become more depressed, thinner, and more leaf-like than they are at *f'*. At the latter point they are separated from each other by distinct intervals, and even at their extremities they have not approached sufficiently entirely to hide the sporangia from view. At their bases these bracts have a breadth of from ·035 to ·04 of an inch.

The vertical sections, figs. 1 and 2, show the thick and succulent character of the disk, and the gradual thinning of the bracteated subdivisions *f*. The appearance of

the prosenchymatous cells composing the bractigerous disk and the lower portions of the bracts is shown in fig. 7.

The Sporangia.—These are arranged, nearly in a single plane, in the concavo-convex space between each two contiguous bractigerous disks, and appear to be grouped in from three to four irregular concentric circles within each space. Each sporangium has a mean diameter of about ·06. The sporangium-wall (*g*) is dense, thick, and cellular; but defective mineralization prevents my ascertaining the exact arrangement of its cells. I have already pointed out that the sporangiophores are so numerous as to suggest the probability that there is a separate one for each sporangium. They arise entirely from the bractigerous disk, and not from the bracts. In fig. 2 we have, at *h*, a tangential section crossing one of these disks; and its upper surface is seen to be crenulated with the intersected bases of these sporangiophores. They are also seen in great numbers, sometimes intersected longitudinally, but more frequently transversely, at *h h* in the sections 3 and 4. Their mean diameter is about ·0075. Fig. 7 represents the base of one of these sporangiophores, seen near the base of the lower right-hand bract of fig. 2 *f''*, but more highly magnified; and fig. 8 exhibits a transverse section of one of them still more enlarged. Both these sections demonstrate their cellular character. They may possibly have contained a few vessels; but I have not succeeded in detecting them. Since these organs all spring from the bractigerous disks, those sustaining the outermost series of sporangia must have required to be much elongated. Figs. 2 *h* and 4 *h'* demonstrate that such is the case. Their transverse section (fig. 8) is usually a circular one. In the tangential section (fig. 6) we discover a row of intersected sporangiophores, *h*, immediately above each disk, and below the sporangia. Fig. 2 *h* further explains this arrangement.

They evidently crept along the upper surface of the disk, as is the case amongst ferns.

In nearly every instance each sporangium is occupied by a cluster of beautiful spores. The appearances which these objects present vary according to the direction of their section, as is well represented in fig. 9. At the first glance, we should be tempted to infer that their exteriors had been spinous; but I have not been able to satisfy myself that such has been the case. I rather conclude that the appearance in question is merely the result of defective mineralization, because, as I have already shown, the outlines of all the more delicate cellular tissues of the specimens are similarly masked by irregular protuberances. The mean diameter of these spores is about ·0037, or nearly the same size as those of the Calamitean strobilus of which I laid a description before this Society last year.

The preceding description will have demonstrated that this fruit is very different from any of which the internal organization has hitherto been described. The differences are not merely of detail, but of type. The question remaining to be answered is an obvious one, viz. to what plant does this fruit belong? The verticillate arrangement of its bractigerous disks and bracts suggests the probability that we must seek for the parent plant amongst such as have their foliage arranged in corresponding verticils; and if this law of association be a sound one, we are apparently shut up to the three genera, *Asterophyllites*, *Annularia*, and *Sphenophyllum*.

But the separation of the first and the second of these genera is of doubtful propriety; at all events I have not yet succeeded in discovering any definite character by which the two can be distinguished. That plants of the Annularian type constituted the delicate aerial foliage of Calamites, appears to be established by so many independent observations as to leave little room for doubting

the conclusion. It does not follow from this, however, that *all* the Asterophyllites and *Annulariæ* are Calamitean. It is possible that, as Dr. Dawson contends, there may be amongst these verticillate types of foliage one group belonging to Calamites and another to some other class of plants more nearly allied to the Ferns. If this latter possibility be converted by further discoveries into an unquestioned fact, we shall have *two* groups of verticillate-foliaged plants amongst which to find those of which our strobili are the fruits, viz. a section of the Asterophyllites and the genus *Sphenophyllum*.

When we compare the internal organization of the fruits themselves, we discover that we now possess three very distinct kinds, but two of them may, notwithstanding generic differences, belong to allied types, if not to the same.

In the fruit which I previously described, and to which reference was made at the beginning of this memoir, the structure of the central axis is almost exactly that of a Calamitean stem, even in its minuter details. In each there is a central cavity lined with cellular tissue and surrounded by a woody zone consisting of a ring of woody wedges, each of which has a longitudinal canal at its inner border; and this zone, in turn, is invested by a second cellular layer which in the true stem becomes a well-defined bark or dermal layer, but which in the axis of the fruit is not capable of being distinguished, the three tissues being there more intimately blended into one than in the stem. The great point upon which I wish to insist is, that the arrangement of these tissues in the axis of the fruit referred to and in the Calamitean stems is so identical, that no reason whatever can be shown why the former should not have been the terminal prolongation of one of the latter.

But when we come to the other two types, viz. that which both Mr. Binney and Mr. Carruthers have identified

with the Calamites and that which I have now described, the case is altogether different. In both these the vascular tissues of the axis occupy the exact centre of the stem, or what would have been the centre of the fistulo-medullary cavity of a Calamite, whilst all their cellular layers are *external* to the vessels. Now, whilst this is an arrangment common enough amongst some Ferns, and still more so amongst the Lycopodiaceæ, *nothing approaching it* has been seen, either amongst the fossil Calamites or amongst the recent Equisetaceæ, with which some of our phytologists are so anxious to identify the Calamites. Mr. Carruthers very correctly acknowledges that "the axis of the strobilus has a bundle of fine scalariform tissue in its centre, forming about a third of its diameter, and generally appearing free from the surrounding cellular tissue, which is composed of somewhat elongated cells." ("On the Structure of the Fruit of Calamites," by Wm. Carruthers, Esq., F.L.S., Journal of Botany, Dec. 1867.)

Now I cannot conceive the possibility of so complete a reversal taking place in the relative positions of the cellular and vascular tissues of the axis as must have occurred if these fruits ever grew upon a Calamitean stem.

In the case of the plant which I have just described, there is no room for doubting that its foliage was arranged in verticils. Mr. Binney's specimens, especially that represented in his pl. vi. fig. 4, are unmistakably associated with the leaves of Asterophyllites; and identical specimens in my own cabinet, apparently belonging to the same type as Mr. Binney's, are similarly associated. It must be remembered also that both Sternberg and Schimper have figured very similar fruits, attached to specimens of the closely allied genus *Sphenophyllum*. Hence it is clear that, if my plant is of the same type as some of those just referred to (and that such is the case there can be no question), we have but a limited range within which to

search for its true affinities. It is the fruit either of *Asterophyllites* or of *Sphenophyllum*; and, judging from the general aspect of its bractigerous disks, I am more disposed to identify it with the former than with the latter. But the important question now returns to us, What are those verticillate plants which are not Calamitean? The first obvious fact is, that they possessed jointed stems. In branched specimens of fructification in my cabinet, the articulations of the central axis are very distinct, and about an inch apart. The stems are flattened by pressure, and exhibit slight indications of longitudinal ridges and furrows, the origin of which I am unable to explain. They are less regular and definite than those of Calamites. The central axis of one of my examples of fruits is six inches in length, giving off six verticils of spikes or strobili, each of which is nearly two inches long; and as I have every reason for supposing that this specimen is only a portion of the original structure, it seems more than probable that the living axis, with its verticillate fructification, may have approached a foot in length. The bractigerous verticils of each strobilus are about the eighth of an inch apart, which is rather more than in our specimen now described; besides which, the latter is broader than the specimens from the shales, so that its spikes have been more compact than in these examples.

The verticillate arrangement of the leaves, and the articulated character of the stems, forbid our associating these plants either with Ferns or with Lycopodiaceæ. On the other hand, the internal organization described in this memoir shows how different they have been from the true Calamites. Yet the two features just referred to obviously indicate closer affinities with the latter genus than with any other of the Carboniferous genera. I have arrived at the conclusion that these plants constituted a distinct family of Asterophyllitaceæ, whose place is near the Cala-

mitaceæ, but occupying a position between the latter group and the Lycopodiaceæ, as indicated by the organization of the central axis, which, as we have seen, is eminently Lycopodiaceous. This conclusion as to the botanical position of these objects has been arrived at from the study of the examples in my own cabinet; but I am glad to find that my friend M. Grand'Eury, of St. Etienne, in France, has arrived at a similar conclusion from the study of specimens found in his own neighbourhood. He ends his reasonings on the subject by affirming, "On peut donc en conclure que les Astérophyllites arborescentes sont issues, non des Calamites, mais des tiges foliifères organisées comme elles" ("Observations sur les Calamites et les Astérophyllites, par M. Grand'Eury." Comptes Rendus, tom. lxviii.). One point of difference between these two types upon which M. Grand'Eury relies has, I think, no existence. He says that the Asterophyllites have "leurs feuilles attachées *au-dessus* des lignes d'articulation et, ce qui est bien autrement significatif, leurs rameaux secondaires insérés à l'aisselle des verticilles foliaires, et non *au-dessous*, ce qui aurait lieu s'ils avaient l'organisation des vraies Calamites" (*loc. cit.*). These supposed differences, as I have shown in a memoir recently laid before the Royal Society of London, do not exist.

I think we may conclude that, of the three types of *Volkmannia* of which we know the organization, two are now collocated with their respective stems with a fair degree of probability. There yet remains the *Volkmannia Binneyii* demanding a parent axis upon which it can hang. This latter strobilus and the primary subject of this memoir obviously belong to two distinct genera of plants. The former has its sporangiophores arranged in verticils of stiff processes, projecting from each alternate node, and at right angles to the axis, the intermediate nodes giving off

bractigerous disks, whose functions are merely protective. The latter gives off a bractigerous disk from each node, as is also the case with my previously described Calamitean strobilus; but the relations which the sporangiophores bear to the sporangia are very different in the example under consideration from what obtains in either of these other cases. The sporangiophores spring in great numbers from the entire upper surface of the bractigerous disk, though not from its peripheral bracts, the sporangia themselves being loosely aggregated; in the Calamitean type each sporangiophore ascending from the bractigerous disk sustained three or four sporangia closely clustered around it, the entire mass of the fruit being rendered very compact through the intense mutual pressure of its sporangia.

Two fruits so different in the details of their structure as *Volkmannia Binneyii* and the subject of this memoir must, as I have already observed, belong to distinct genera; consequently it is desirable to separate them. This can readily be done by accepting Professor Schimper's generic name, calling the one fruit *Calamostachys Binneyana*, and retaining the other, for the present, in the provisional genus *Volkmannia*. But as the specific features of the latter are very distinctive, and as I cannot identify it *specifically* with those forms of which the external aspect alone has been so often described, I propose to distinguish it by the name of *Volkmannia Dawsoni*, associating with it the name of the eminent geologist who has contributed so much to our knowledge of Canadian palæophytology.

I may observe in conclusion that, though *Calamostachys Binneyana* exhibits features which mark strongly the generic differences between it and *Volkmannia Dawsoni*, the organization of its central axis brings it much nearer to the latter plant than to the Calamites, with which Mr. Carruthers and others have associated it.

INDEX TO PLATES I., II., and III.

(VOLKMANNIA DAWSONI.)

a. Central vascular bundle of the axis.

b. Outer cellular wall of the axis.

c. Middle tissue of the axis, probably cellular.

d. Verticillate bractigerous disks.

e. Sulcated inferior surface of the peripheral part of the bractigerous disk, indicating its approaching division into succulent bracts.

f. Thick primary bracts, resulting from the division of the bractigerous disks.

f'. Middle portion of the bracts, where they exhibit an oval transverse section.

f''. Extremities of the depressed bracts, exhibiting an oblong linear transverse section.

g. Walls of the sporangia.

h. Sporangiophores.

x x (fig. 1). Line of the transverse section fig. 3.

y y. Line of the transverse section fig. 4, only from a part of the strobilus on the right hand, where sufficiently perfect to cross the line of section.

z z. Line of the tangential section fig. 6.

Pl. II.

Fig. 1. Vertical section through the centre of the axis.

2. Oblique tangential section, crossing the bractigerous disk at its upper part, and passing between the outer cellular cylinder of the axis and its central vessels inferiorly.

Pl. I.

Fig. 3. Slightly oblique transverse section, made in the plane of the line *x x* of fig. 1.

4. Similar section, made in the plane of the line *y y*, fig. 1.

Pl. III.

Fig. 5. Enlarged representation of the central vascular axis of fig. 3.

6. Tangential section, made in the plane of the line *z z* of fig. 1. being intermediate between the section fig. 2 and the exterior of the strobilus.

7. Portion of the lowest bractigerous disk on the right of fig. 2, giving off a sporangiophore (*h*) from its upper surface, and exhibiting the prosenchymatous structure of the former organs.

Pl. II.

Fig. 8. Transverse section of a sporangiophore.

9. Three aspects of the spores.

The same letters are employed to indicate identical structures in all the above figures.

IV. *Notes on the Botany of Mere, Cheshire.*

By George E. Hunt, Esq.

Read November 29th, 1870.

The border of Mere Mere has for long been a locality famous to the botanists round Manchester.

The first published Manchester floras bore its name as the habitat of the rare *Elatine hexandra* and *Limosella aquatica*.

In 1855 Mr. Wilson's 'Bryologia Britannica' gave a still greater notability to the place by the record of several extremely rare mosses from thence—among others, of *Physcomitrium sphæricum*, which is thus recorded by him:—

"On the dried mud of pools, Mere, Cheshire, September 1834, W. Wilson; not found in any subsequent year; the only known locality in Britain."

The following are also recorded in the same work as occurring at Mere:—

Phascum serratum β.
„ *sessile*.
„ *rostellatum*.

I was led in 1864 by these various notices to commence a systematic and continuous exploration of Mere, with the view of discovering as many of the recorded mosses as might still exist there. Some of them being exceedingly minute, it has taken a considerable time to detect all; and it may be of service to other bryologists in the district to mention those which grow there at the present date, and also the nature of soil which they prefer.

1. *Physcomitrium sphæricum*. A careful search in 1864 led to the rediscovery of this species in very minute quan-

tity; in 1865 it was still more sparing (not above a dozen capsules); 1866 was so exceedingly wet a season, that the plant could not have come up at all; in 1867 it again occurred very sparingly; in 1868 it was plentiful, but destroyed by the autumn rains before much of the fruit had ripened; in 1869 again frequent, and would have been plentiful, but the autumn rains again destroyed it whilst the fruit was even more immature than in the preceding year; in 1870 very plentiful, and abundance of it has come to maturity. This moss *always* grows on dried mud.

2. *Phascum serratum* β is frequent every autumn on clay and sandy banks at Mere. It occurs quite frequently in cornfields at Bowdon, in damp seasons, coming up a few weeks after the corn has been cut. In cornfields at Bowdon its companions are *Phascum muticum, Phascum alternifolium,* and *Pottia truncata,* and very rarely *Trichodon cylindricus.* The latter never fruits in this district.

3. *Phascum nitidum,* frequent every autumn at Mere, on clay and sandy banks; it occurs elsewhere about Bowdon on newly cut ditch-banks.

4. *Phascum rostellatum.* On banks at Mere, with the two previous species, but much more sparingly. It has also been found in Sussex by Mr. Mitten, and was collected there again last year by Mr. Davies. It is one of the rarest of all the British mosses.

5. *Phascum sessile.* Very rare at Mere. I collected it in the autumn of 1869, and again in November 1870, intermixed very sparingly among *Phascum serratum,* from which it is difficult to separate it, except with the aid of the microscope; with this it can be at once distinguished from that species by its longer, more rigid, almost entire leaves, with a very wide nerve. *Phascum serratum* has no nerve; and the leaves are spinulosely serrated. *Phascum sessile* was gathered in Sussex many years since;

but I have not heard of its recent discovery, either there or elsewhere. It is one of the rarest British mosses.

6. *Phascum patens.* On dried mud, almost every season, intermixed with *Physcomitrium sphæricum* and usually much more plentiful than that species. This moss comes up in autumn in the Ashle district of Bowdon, although very sparingly, wherever an open drain has been cut in spring. It also springs up about Bollington, under the same circumstances.

7. *Phascum cuspidatum.* I have not yet found this *at* Mere; but it comes up on banks on the Chester road, between Bowdon and Bucklow Hill, when they have been newly made up, or plastered with mud from the road.

8. *Leskia polycarpa* fruits freely about the roots of trees on the borders of Mere, both in autumn and spring.

9. *Hypnum riparium.* A very neat variety of this moss fruits in abundance in August and April on clay banks and at the roots of trees at Mere.

Hepaticæ.

Riccia fluitans and *crystallina* are both frequent on dried mud at Mere, with *Phascum patens* &c., and both species fruit freely there.

Numerous interesting flowering plants are also found, viz. :—

Elatine hexandra,
Limosella aquatica,
Peplis portula,
Polygonum minus,
Littorella lacustris,
} All plentiful on mud.

Carex vesicaria. Fringing the woods at the edge of the Mere.

Scirpus acicularis. In vast quantity in sandy places.

Carex Œderi. In stony and grassy places. This is the

true *Œderi*, and very rare. I have only seen it elsewhere on the sands on the south side of Southport, where it is very abundant and luxuriant. It appears quite distinct as a species from *C. flava* (including *C. lepidocarpa*), with which it is often placed as a variety.

Centunculus minimus. Frequent some seasons in the open pastures on the borders of the Mere.

Mentha sativa. In ditches by the road-side, between Bucklow Hill and Mere Mere.

Rubus Balfourianus.
Rubus pallidus. } In thickets by the Mere.

Polygonum mite has been reported from Mere; but, after searching without success for it for several seasons, I can only suppose that some of the more luxuriant forms of *P. minus* frequent there have been mistaken for it. The seeds of *P. minus*, which are *shining* black, and only half the size of those of *P. mite*, afford the only safe distinction.

V. *The Tails of Comets, the Solar Corona, and the Aurora, considered as Electric Phenomena.* Part I. By Professor Osborne Reynolds, M.A.

Read November 29th, 1870.

Although the tails of comets are usually assumed to be material appendages which accompany these bodies in their flight through the heavens (and the appearance they present certainly warrants such an assumption), yet this is not the only way in which these tails may be accounted for. They may be simply an effect produced by the comet on the material through which it is passing, an effect

analogous to that which we sometimes see produced by a very small insect on the surface of still water. We see a dark spot, and on looking closer we find a small fly or moth flapping its wings and creating a disturbance which was visible before the insect which produced it.

There is nothing else that we can conceive their tails to be; so that they must be one or the other of these two things,—either

(1) Material appendages of the nucleus, whether the material be limited to the illuminated tail or surround the comet on all sides—or

(2) Matter which exists independently of the comet, and on which the comet exerts such a physical influence as to render it visible.

Respecting the composition of these bodies Sir John Herschel says:—"There is beyond question some profound secret and mystery of nature concerned in the phenomenon of their tails. Perhaps it is not too much to hope that future observation, borrowing every aid from rational speculation, grounded on the progress of physical science generally (especially those branches of it which relate to the ætherial or imponderable elements), may ere long enable us to penetrate this mystery, and to declare whether it is matter in the ordinary acceptation of the term that is projected from their heads with such extravagant velocities, and if not impelled at least directed in its course by reference to the sun a as point of avoidance. In no respect is the question as to the materiality of the tail more forcibly pressed on us for consideration than in that of the enormous sweep which it makes round the sun in perihelio, in the manner of a straight and rigid rod, in defiance of the law of gravitation, nay, even of the received laws of motion, extending (as we have seen in the comets of 1680 and 1843) from near the sun's surface to the earth's orbit, yet whirled round unbroken: in the latter

case through an angle of 180° in little more than two hours. It seems utterly incredible that in such a case it is one and the same material object which is thus brandished. If there could be conceived such a thing as a *negative shadow*, a momentary impression made upon the luminiferous æther behind the comet, this would represent in some degree the conception such a phenomenon irresistibly calls up. But this is not all. Even such an extraordinary excitement of the æther, conceive it as we will, will afford no account of the projection of lateral streamers, of the effusion of light from the nucleus of the comet towards the sun and its subsequent rejection, of the irregular and capricious mode in which that effusion has been seen to take place, none of the clear indications of alternate evaporation and condensation going on in the immense regions of space occupied by the tail and coma—none, in short, of innumerable other facts which link themselves with almost equally irresistible cogency to our ordinary notions of matter and force."

There can be no doubt that, if these tails are matter moving with the comet, this matter must be endowed with properties such as we not only have no experience of, but of which we can form no conception. This would almost seem a sufficient reason for rejecting the first hypothesis. Moreover, on the second hypothesis there is no difficulty in the immense velocity with which these tails are projected from the head or whirled round when the comet is in perihelio; for, to take the "negative shadow" as an illustration, here we should have a velocity of projection equal to that of light, and the only effect of the whirling would be a slight lagging in the extremity of the tail, causing curvature similar to that which actually exists; and whatever the action may be, if its velocity of emission or transmission be sufficiently great, this effect will be the same. But whether this hypothesis is to be rejected be-

cause involving assumptions beyond conception or contrary to experience, must depend on the answers to the following question :—Do we know, or can we conceive, any physical state into which any substance which can be conceived to occupy the space traversed by comets could possibly be brought so as to make it present the appearance exhibited by comets?

Now I think the answer must be in the affirmative, and that we may leave out the terms conceive and conceivable. For electricity is a well known state, and gases are well known substances; and when electricity under certain conditions, as in Dr. Geissler's tubes, is made to traverse exceedingly rare gas, the appearance produced is similar to that of the comets' tails; the rarer this gas is, the more susceptible is it of such a state; and, so far as we know, there is no limit to the extent of gas that may be so illuminated. Hence we may suppose the exciting cause to be electricity, and the material on which it acts and and which fills space to have the same properties as those possessed by gas. What is more, we can conceive the sun to be in such a condition as to produce that influence on this electricity which should cause the tail to occupy the direction it does; for such an electric discharge will be powerfully repelled by any body charged with similar electricity in its neighbourhood.

The electricity would be discharged by the comets on account of some influence which the sun may have on them, such an influence being well within the limits of our conception.

The appearances of the comet in detail, such as the emission of jets of light towards the sun and the form of the illuminated envelope, are all such as would necessarily accompany such an electrical discharge.

In fact, if the possibility of such a discharge is admitted, I believe it will explain all the phenomena of comets.

As to the possibility, or even the probability, of such a discharge, I think it may be established on very good grounds.

The tails of comets may or may not be one with their heads; but whichever is the case, it is certain that the difference in the appearance of comets and of planets indicates some essential difference either in the materials of which these bodies are respectively composed, or else in the conditions under which their materials exist. Now, from the motion of comets, we know that their heads follow the same laws of motion and gravitation as all other matter; and therefore we have good evidence, so far as it goes, that comets and planets are similarly constituted as regards materials. And since the appearance of a comet changes very much as it passes round the sun, any assumptions with regard to the material of comets in order to account for their difference from planets would not account for the variety of appearance the same comet presents at different times. On the other hand the conditions of comets and planets must necessarily be very different, from the extreme difference in the shapes of the orbits they describe. Each planet remains nearly at a constant distance from the sun (whatever that distance may be), so that the heat or any physical effect the sun may have upon it will also be constant; on the comets its action must change rapidly from time to time, particularly when the comet is in certain parts of its orbit. Hence we may say that the temperature and general physical condition of planets is nearly constant, and that of comets, for the most part, continually varying.

There is, too, a very remarkable connexion between the appearance of the comet and the rate at which the sun's action on it changes. Herschel says:—"Sometimes they first make their appearance as faint and slow-moving objects, with little or no tail, but by degrees accelerate, en-

large, and throw out from them this appendage, which increases in length and brightness till (as always happens in such cases) they approach the sun and are lost in his beams. After a time they again emerge on the other side, receding from the sun with a velocity at first rapid, but gradually decreasing. It is, for the most part, after thus passing the sun that they shine forth in all their splendour, and their tails acquire their greatest length and development, thus indicating plainly the sun's rays as the exciting cause of that extraordinary emanation. As they continue to recede from the sun their motion diminishes and their tail dies away, or is absorbed into the head, which itself grows continually feebler, and is at length altogether lost sight of."

Here, although unconsciously, Herschel has connected the increase of brightness with the increase of speed with which comets approach the sun, and the diminution in brightness with the diminution of the velocity with which they leave the sun. And although from Herschel's remark just quoted it might be inferred that proximity to the sun is the cause of the increase of brightness, this is proved not to be the case; for (as in the case of Halley's comet) when near its perihelion the tail sometimes dies away, and the comet shrinks. In such cases, when the comet is nearest to the sun there is no development of tail, which shows clearly that it is not the intensity of the sun's rays, but the change in their intensity, that is the exciting cause of these extraordinary appearances; so that there is no reason to suppose that a planet composed of the same material as a comet, no matter how close to the sun, would show a vestige of tail or other cometic appearance.

It is, then, to this change in position that we must attribute those peculiar appearances which belong to comets.

Now is not electricity the very effect which would

naturally result from such a state of change and variation in condition?

A. de la Rive remarks, "Electricity is one of the most frequent forms which the forces of nature assume in their transformations." It certainly often accompanies a change in temperature. There is every indication that it is so in our atmosphere; for the times when its intensity is a maximum are just after sunrise and just after sunset, both winter and summer.

For these reasons it seems to me not only possible but probable that these strange visitors to our system are clothed in electrical garments with which the regular inhabitants are unacquainted.

The electricity must after all depend on the composition of the comet; for known substances do not all show the same electrical properties. Hence, by assuming comets to be composed of various materials, we have a source to which we can attribute the different appearances presented by the different individuals. To the same source we may attribute the irregularity in the direction of their tails, and the lateral streamers they occasionally send out.

Secondly, I think this electrical hypothesis is supported by the, to me, seeming analogy between comets, the corona, and the aurora—an analogy which suggests that they must all be due to the same cause. They may be all described as streams of light or streamers, having their starting point more or less undefined, and traversing spaces of such extent and with such velocities as entirely to preclude the possibility of their being material in any sense of that word with which we are acquainted.

The aurora has long been considered an electric phenomenon; and recently the same effect has been produced by the discharge of electricity of very great intensity through a very rare gas, there being no limit to the space which it will thus traverse. This being so, why should

not the tails of comets and the corona also be electric phenomena? Their appearance and behaviour correspond exactly with those of the aurora; and there is surely nothing very difficult in imagining the sun, which is the source of so much heat, being also the source of some electricity. Neither will there appear any thing wonderful in the electricity of comets when we consider that of the earth. We must not look on our inability to explain the cause of such an electric discharge as fatal to its existence; for we cannot any more explain the existence of the electricity which causes the aurora. If we cannot explain whence these electricities come, we can at least show that the conditions which are most favourable to the development of the aurora exist in much greater force on the comets than they do on the earth. The greatest development of the aurora borealis takes place at the equinoxes. There is a cessation in summer, and another in winter. Now the equinoxes are the times when the action of the sun on our northern hemisphere is changing most rapidly. Hence the condition favourable for the aurora is change in the action of the sun. The same thing is pointed out by the diurnal variation in the electricity of the atmosphere; for, as has been already shown, the change in temperature on the comets is incomparably greater than it is on the earth, and its variation corresponds with the variation in the atmosphere of the comet.

Ångström has also shown that the light from the aurora, the corona, and the zodiacal light are all of the same character, or all give the same bright lines when viewed through the spectroscope, and that these lines correspond to the light from no known substance. This indicates that, whatever this light may be, the incandescent material is the same in all cases; or may we not assume that it is the medium which fills space that is illuminated by the electric discharges? This would be supported by the fact that the

light from the heads of two small comets indicated carbon, whereas that from the tails only gave a faint continuous spectrum. For an electric discharge would first illuminate the atmosphere of the comet, or even carry some of the solid material off in a state of vapour, and then pass off to the surrounding medium; thus, while the spectrum from the head would be that of cometary matter, the tail would be due to the incandescent ether.

I would here suggest that gas, when rendered incandescent by electricity, may reflect light; (it will certainly cast a shadow from the electric light;) and if this be the case, part of the light from comets' tails may after all be reflected sunlight.

At any rate, it is certain that the appearance of streamers, the rapidity of change and emission, the perfect transparency, and the wave-like fluctuations which belong to these phenomena are all exhibited by the electric brush; in fact the electric brush will explain all these appearances, which have defied all attempts at explanation on a material hypothesis.

I have only to add that the main assumption involved in the electric theory is, that space is occupied by matter having similar electrical properties to those of gas; and I would ask, is it not more rational to make such an assumption than it is to attribute unknown and inconceivable properties to cometary matter?

Theories, even, if founded only on rational speculation, often, I believe, prove very useful, inasmuch as they afford observers a definite purpose in their speculations—something to look for, something to establish or to refute; and I publish these speculations of mine at this particular moment in the hope that they may perchance serve such a purpose.

VI. *The Tails of Comets, the Solar Corona, and the Aurora, considered as Electric Phenomena.* Part II. By Professor Osborne Reynolds, M.A.

Read February 7th, 1871.

In the paper which I read before this Society on the 29th of November last, I endeavoured to show that it is probable that these phenomena are a species of that action known as the electric brush, taking place in the medium which fills space, be it ether, or simply gas, or both. The reasoning I made use of was essentially *a fortiori*. I pointed to the fact that the electric brush as seen in the Geissler tubes exhibits similar appearances, and that at the times of greatest display on the part of comets and the aurora similar conditions are present, such as a change in the action of the sun, conditions which, to say nothing more, are favourable to electric disturbance. I purposely avoided all attempts to explain how the brush may be produced, feeling that it was sufficient to point to the aurora, which is universally admitted to be electrical, as a proof that such phenomena do exist, even if we cannot explain how. This proof, however, is perhaps not quite satisfactory. In order that it might be complete, the other phenomena would have to be produced in the same way as the aurora; and this, although possible, is not necessary. An assumption which is commonly made respecting the phenomena of the aurora cannot be made with respect to the others. This assumption assigns the two magnetic poles of the earth as the two electrodes between which the electric discharge takes place which forms the aurora borealis and the australis. If this assumption be maintained, some other explanation must be found for the manner in which electricity may form the tails of comets

and the corona. It is quite clear that the tail of a comet cannot be due to a discharge between two electrodes situated on the comet itself. In the same way, from the position occupied by the corona, it can hardly be due to electricity passing between two electrodes on the sun. In fact, if a comet's tail is electrical, it is due to a discharge of electricity of one kind or another from the comet, which for the time answers to one of the electrodes only. The same may be said of the corona and the sun. If we could observe the aurora from a point distant from the earth, it is very probable that we should find the same to be the case; but whether this would be so or not, an assumption has been made as to the cause and nature of the aurora which will answer just as well for the corona and comets' tails: it is, that the sun, acting by evaporation or otherwise, causes continual electric disturbance between the earth and its atmosphere, the solid earth being negatively charged and the atmosphere positively, and that the aurora is the reunion of these electricities taking place in the atmosphere.

Now, as has been already said, this assumption will serve for the comets and the sun as well as for the aurora. If there is a continual electric disturbance between the sun and the medium in which it is placed, so that the sun becomes negatively and the medium positively charged, the reunion of these electricities would form the corona. It must not be supposed that I assume the sun to be a reservoir of electricity which it is continually pouring into space. I consider that the supply of electricity in the sun is kept up by some physical action going on between the sun and the medium of space, whereby the sun becomes negatively charged, and the medium positively.

This may be well illustrated by reference to the common electrical machine: here the motion of the glass against the rubber causes the glass to become positively and the

rubber negatively charged; and these electricities do not unite instantly there and then, but remain and accumulate in the respective bodies, until collected and brought together again by the conductor.

Assume, then, that the sun is in the position of the rubber, while the ether is in that of the glass; then the corona corresponds to the spark or brush which leaves the conductor. On the same assumption the negative electricity of the comet would be more and more set free by the inductive action of the sun as the comet approached it, and would also be driven off by induction in a direction opposite to that of the sun—and, combining with the positive electricity in the ether, would form the tail of the comet, in a manner analogous to that in which a native spark is given off by the lid of the electrophorus.

I think that a rational account may in this way be given of the manner of the electrical action to which I have attributed these phenomena; but I do not consider that the probability of the truth of this electrical hypothesis depends on the value of such an explanation. It is an assumption, based on the manner in which it fits into its place and explains the appearances presented by these beautiful phenomena.

Since this paper was written, my attention has been called to the fact that Mr. Richard Proctor has published views of these phenomena which somewhat resemble mine. He attributes them in part to electricity, and in part to meteors. There is, however, this fundamental difference between our views—that he regards the tails of comets as consisting of cometary matter, the difficulty of conceiving which was the origin of these speculations. Moreover I can conceive no electric discharge between two meteors without a medium between them; and if there is a medium, why is there any necessity for meteors? If, as I see good reason to suppose, gas, when glowing with

electricity, reflects or scatters rather than absorbs light of the wave-length which it radiates, that portion of the coronal light which is polarized and assumed to be reflected, will be accounted for. I think that recent observations have confirmed the probability of these speculations, inasmuch as they have confirmed the facts on which these speculations were based. There is one point which has not been already noticed, but which seems to me to be of some importance.

If the corona be an electric discharge, the electricity will be continually carrying off some of the elements of the sun into space, where they will be deposited and condensed. May not this stream of matter be the cause of the existence of small meteors, and supply the place of those which continually fall into the larger bodies?

VII. *On the Rainfall at Old Trafford, and Comparison with the Averages of Twenty Years and Seventy-seven Years.* By G. V. Vernon, F.R.A.S., F.M.S.

Read before the Physical and Mathematical Section, April 25th, 1871.

In a paper published in Volume I. of the Third Series of the Society's Memoirs, I gave the rainfall for Old Trafford for the years 1850 to 1860, and I beg now to submit to the Society a continuation of the same down to the end of 1870, making a period of ten years.

The period 1850 to 1860 was unfortunately very incomplete, owing to the month of August being deficient in the first six years of the period. The series I now submit is complete throughout the period of ten years.

In the first place I annex a comparative statement of the various periods.

Month.	1850–1860.	1861–1870.	1850–1870.	1794–1870.
	in.	in.	in.	in.
January	2·778	2·659	2·719	2·505
February	1·899	2·412	2·156	2·397
March	1·925	2·356	2·131	2·297
April	1·848	1·983	1·915	2·031
May	1·808	2·112	1·953	2·316
June	3·310	2·286	2·798	2·683
July	2·869	2·547	2·713	3·515
September	4·806	2·913	3·454	3·534
August*	2·745	4·014	3·349	3·263
October	3·280	4·191	3·785	3·877
November	2·611	3·245	2·928	3·814
December	2·786	3·347	3·053	3·307
Sums	32·665	34·165	32·954	35·539

This Table evidently points out the fact that the period 1850 to 1860 was drier than the period 1860 to 1870, and would have shown it much more so if the observations for August had been complete during the earlier period, as the average of the 4 years 1857, 1858, 1859, and 1860 is quite an abnormal value, the rainfall in this month being excessive in each of these years.

During the last ten years, the smallest amount of rain occurred in 1865, the amount being 29·389 inches, falling upon 164 days; the largest amount occurred the following year, 1866, the amount being 43·169 inches, falling upon 214 days.

From the longer period of observations, 1794 to 1870, it seems to be quite well established that in this district the minimum rainfall occurs in April, and the maximum in October; and, but for the departure from symmetry in September, we should have a simple curve, with one maximum and one minimum.

In October 1870, the largest fall of rain for the month occurred of any October between 1794 and 1870, and during

* 1850 to 1860, four years only.

this entire period there were only two months approaching so large a fall, viz. July 1828, 11·280 inches, and August 1799, 8·740 inches. (Society's Memoirs, 2nd Ser., vol. xv.)

The summer months of 1868, 1869, and 1870 were excessively dry. The four months of May, June, July, and August had a total rainfall of, 1868, 4·194 inches; 1869, 7·612 inches; 1870, 4·997 inches; the average for 77 years being 12·048, showing a very great falling off, especially in 1868 and 1870.

In trying to trace some law of periodicity in this long series of rainfall observations, I have not been able to find any defined period.

Going back to 1786, it would appear from Mr. Walker's rainfall returns, 1786–1793, that the heaviest rainfalls did not occur at the period of lowest annual mean temperature, but a few years later; but the irregularities are so great, that some other method must be adopted to reduce the irregularities of the temperature- and rain-curves for the year: perhaps taking five years' means might enable a better comparision to be made; that is, the year itself and the two preceding and two following ones in each case.

Rainfall at Old Trafford, Manchester.

	January.			February.		
Year.	Rainfall.	Differ. of 21 years.	Differ. of 77 years.	Rainfall.	Differ. of 21 years.	Differ. of 77 years.
	in.	in.	in.	in.	in.	in.
1861	0·388	−2·331	−2.117	2·522	+0·366	+0·125
1862	1·896	−0·823	−0·609	0·958	−1·198	−1·439
1863	4·425	+1·706	+1·920	0·941	−1·215	−1·456
1864	1·684	−1·035	−0·821	4·027	+1·871	+1·630
1865	3·112	+0·393	+0·607	2·357	+0·201	−0·040
1866	3·252	+0·533	+0·747	2·983	+0·827	+0·586
1867	3·270	+0·551	+0·765	2·930	+0·774	+0·533
1868	2·746	+0·027	+0·241	2·114	−0·042	−0·283
1869	2·686	−0·033	+0·181	4·436	+2·280	+2·039
1870	3·131	+0·412	+0·626	0·856	−1·300	−1·541
Means.	2·659	2·719	2·505	2·412	2·156	2·397

Rainfall at Old Trafford, Manchester.

	March.			April.		
Year.	Rainfall.	Differ. of 21 years.	Differ. of 77 years.	Rainfall.	Differ. of 21 years.	Differ. of 77 years.
	in.	in.	in.	in.	in.	in.
1861.	4·143	+2·012	+1·846	2·426	+0·511	+0·395
1862.	3·669	+1·538	+1·372	2·717	+0·802	+0·686
1863.	0·804	−1·327	−1·493	1·391	−0·524	−0·640
1864.	2·011	−0·120	−0·286	1·602	−0·313	−0·429
1865.	1·674	−0·457	−0·623	1·082	−0·833	−0·949
1866.	2·168	+0·037	−0·129	0·299	−1·616	−1·732
1867.	1·446	−0·685	−0·851	4·323	+2·408	+2·292
1868.	3·999	+1·868	+1·702	1·676	−0·239	−0·355
1869.	1·270	+0·039	−1·027	2·096	+0·181	+0·065
1870.	2·378	+0·247	+0·081	2·217	+0·302	+0·186
Means.	2·356	2·131	2·297	1·983	1·915	2·031

	May.			June.		
Year.	Rainfall.	Differ. of 21 years.	Differ. of 77 years.	Rainfall.	Differ. of 21 years.	Differ. of 77 years.
	in.	in.	in.	in.	in.	in.
1861.	0·734	−1·219	−1·582	2·412	−0·386	−0·271
1862.	4·470	+2·517	+2·154	3·072	+0·274	+0·389
1863.	1·724	−0·229	−0·592	4·631	+1·833	+1·948
1864.	3·175	+1·222	+0·859	2·945	+0·147	+0·262
1865.	3·187	+1·134	+0·871	0·957	−1·841	−1·726
1866.	1·540	−0·413	−0·776	3·975	+1·177	+1·292
1867.	1·950	−0·003	−0·366	1·591	−1·207	−1·092
1868.	0·872	−1·081	−1·544	0·368	−2·430	−2·315
1869.	2·726	+0·773	+0·410	1·122	−1·676	−1·561
1870.	0·746	−1·207	−1·570	1·790	−1·008	−0·893
Means.	2·112	1·953	2·316	2·286	2·798	2·683

Rainfall at Old Trafford, Manchester.

	July.			August.		
Year.	Rainfall.	Differ. of 21 years.	Differ. of 77 years.	Rainfall.	Differ. of 14 years.	Differ. of 77 years.
	in.	in.	in.	in.	in.	in.
1861.	3·646	+0·933	+0·131	2·232	−1·222	−1·302
1862.	4·527	+1·814	+1·012	2·350	−1·104	−1·184
1863.	1·630	−1·083	−1·885	5·027	+1·573	+1·493
1864.	1·687	−1·026	−1·828	2·367	−1·087	−1·167
1865.	2·996	+0·283	−0·519	3·840	+0·386	+0·306
1866.	4·309	+1·596	+0·794	5·119	+1·665	+1·585
1867.	4·284	+1·571	+0·769	1·410	−2·044	−2·124
1868.	0·454	−2·259	−3·061	2·500	−0·954	−1·034
1869.	1·131	−1·582	−2·384	2·633	−0·821	−0·901
1870.	0·809	−1·904	−2·706	1·652	−1·802	−1·882
Means.	2·547	2·713	3·515	2·913	3·454	3·534

	September.			October.		
Year.	Rainfall.	Differ. of 21 years.	Differ. of 77 years.	Rainfall.	Differ. of 21 years.	Differ. of 77 years.
	in.	in.	in.	in.	in.	in.
1861.	4·050	+0·701	+0·787	1·230	−2·555	−2·647
1862.	4·998	+1·649	+1·735	5·035	+1·250	+1·158
1863	5·559	+2·210	+2·296	6·242	+2·457	+2·365
1864.	4·009	+0·660	+0·746	1·908	−1·877	−1·969
1865.	0·6 6	−2·683	−2·597	5·005	+1·220	+1·128
1866.	7·128	+3·779	+3·865	2·521	−1·264	−1·356
1867.	2·990	−0·539	−0·273	3·979	+0·194	+0·102
1868.	1·760	−1·589	−1·503	4·505	+0·720	+0·628
1869.	6·320	+2·971	+3·057	3·119	−0·666	−0·758
1870.	2·657	−0·692	−0·606	8·363	+4·578	+4·486
Means.	4·014	3·349	3·263	4·191	3·785	3·877

Rainfall at Old Trafford, Manchester.

	November.			December.		
Year.	Rainfall.	Differ. of 21 years.	Differ. of 77 years.	Rainfall.	Differ. of 21 years.	Differ. of 77 years.
	in.	in.	in.	in.	in.	in.
1861.	3·878	+0·950	+0·064	2·066	−0·987	−1·241
1862.	1·685	−1·243	−2·129	3·221	+0·168	−0·086
1863.	2·902	−0·026	−0·912	3·064	+0·011	+0·057
1864.	3·255	+0·627	−0·559	1·970	−1·083	−1·337
1865.	2·770	−0·158	−1·044	0·743	−2·310	−2·564
1866.	5·721	+2·793	+1·907	4·154	+1·101	+0·847
1867.	2·431	−0·497	−1·383	3·975	+0·922	+0·668
1868.	3·108	+0·180	−0·706	8·123	+5·070	+4·816
1869.	4·284	+1·356	+0·470	3·623	+0·570	+0·316
1860.	2·420	−0·508	−1·394	2·532	−0·521	−0·775
Means	3·245	2·928	3·814	3·347	3·053	3·307

Yearly fall.

	in.	days.		in.	days.
1861	29·727	199	1866	43·169	214
1862	38·598	218	1867	34·579	188
1863	38·340	215	1868	32·225	188
1864	30·640	171	1869	35·446	197
1865	29·389	164	1870	29·551	155

VIII. *Contributions to our knowledge of the Antimony Oxychlorides.* By William Carleton Williams, Dalton Scholar in the Laboratory of Owens College. Communicated by Professor Roscoe, F.R.S.

Read October 17th, 1871.

Phosphorus oxychloride, $PO Cl_3$, having been prepared by heating together one molecule of phosphorus pentoxide with three of pentachloride, it appeared not unlikely that

a similar reaction might occur with antimony, giving rise to the missing oxychloride corresponding to the phosphorus compound above mentioned.

The following investigation was undertaken at Dr. Roscoe's request, with the view of elucidating the above reaction, as no oxychlorides derived from the pentachloride have as yet been described.

A mixture of one molecule of antimony pentoxide (prepared by heating the pentachloride with water) with three molecules of the pentachloride was heated for some hours in sealed tubes to 140° C. On opening the tube after cooling, it was found to contain, besides unchanged pentachloride and pentoxide, two distinct solid crystalline compounds. When the pentoxide prepared by the action of nitric acid on the metal is heated with the pentachloride in a similar way, no oxychloride is formed.

One of these compounds fuses at 85° C. to a clear yellowish liquid, whilst the other, produced only in small quantities, is found in minute yellowish crystals adhering to the top of the tube; and these fuse at a higher temperature. In order to obtain the first of these substances in a pure state, it is sufficient to place the tube upright in a vessel of water at 90°, with the empty end downwards; the fusible oxychloride then melts, and collects as a perfectly clear yellowish liquid; after cooling, the tube is opened; and the small quantity of residual pentachloride having been poured off, the solid mass is dried on a porous plate over solid caustic potash *in vacuo*. The oxychloride thus obtained is a perfectly white crystalline substance, exceedingly hygroscopic, so that when exposed to the air for a few minutes it becomes a pasty mass which rapidly changes to a liquid. It readily dissolves in an aqueous solution of tartaric acid; whilst it is decomposed by water, and is perfectly insoluble in carbon disulphide. The melting-point of the substance is 85° C., as a mean of well-

agreeing determinations made with four different preparations. When heated in a retort until it boils, chlorine gas is evolved, and, whilst pentachloride and trichloride of antimony distil over, a residue of antimony pentoxide remains in the retort. A modification of Rose's well-known method of precipitation, first as insoluble antimoniate of soda, and then as antimony sulphide, was employed for the determination of the antimony. The precipitated sulphide was (1) oxidized to Sb_2O_4, either by treatment with pure fuming nitric acid, or by heating with from ten to twenty times its weight of pure mercuric oxide; and (2) the sulphide was completely reduced to metallic antimony by heating gently in a current of hydrogen until sulphuretted hydrogen ceased to be evolved. In the estimation of chlorine, it was found that, when silver nitrate is added to a solution of an antimony oxychloride acidified by nitric acid, a small trace of antimony is invariably carried down with the silver chloride. In order to free the precipitate from antimony, the silver chloride is first heated gently in a current of hydrogen, when the silver is reduced; and on stronger ignition, the whole of the antimony is volatilized as the hydrogen compound. Thus 1·277 grm. of an alloy, containing 2·5 parts of antimony to 97·5 parts of silver, was found to lose on heating in hydrogen 0·0321 grm., corresponding to 97·48 per cent. of silver.

The accuracy of each of the above methods was tested by determining the percentage of antimony and chlorine in pure antimony trichloride, the results agreeing closely with each other and with the theoretical composition. The objection to Schaeffer's method of decomposing the oxychloride by boiling with a solution of sodium carbonate is, that the precipitated oxide of antimony being in a very finely divided state, a portion of it is apt to pass through the filter on washing.

The following are the results of analysis made with three

specimens of the oxychloride, melting at 85°, which were prepared on different occasions :—

(A.) *Antimony-determinations.*

Preparation.	Method.	Weight of substance taken.	Total wt. sulphide +sulphur.	Do. taken for oxidation.	Sb_2O_4 found.	Percent. of antimony.
No. 1.	1.	0.9940	1·416	1·2980	0·500	43·47
No. 1.	1.	0·4455	0·299	0·2460	0·2005	43·34
No. 2.	1.	0·2490	0·168	0·1240	0.1005 (metal)	43·37
No. 2.	2.	0·6225	0·483	0·3070	0·172	43·47
No. 3.	1.	0·4630	0·345	0·2815	0·2085	43·67

Mean of five antimony-determinations, 43·46 per cent. Sb.

(B.) *Chlorine-determinations.*

The chlorine was estimated (1) in the tartaric acid solution of the substance by the method above described, and (2) in the filtrate after precipitating the antimony with sulphuretted hydrogen.

Preparation.	Weight of substance taken.	Weight of Ag Cl precipitated.	Wt. of Ag Cl taken for reduction.	Ag found.	Percent. of chlorine.
1.	0·3975	0·881	0·848	0·6370	54·72
1.	0·3405	0·758	0·714	0·5355	54·88
1.	0·3630	0·796	0·006	.....	54·65
2.	0·7385	1·6325	0·0065	.....	54·71
3.	0·3630	0·798	0·006	.....	54·79

Mean of five chlorine-determinations, 54·75 per cent. Cl.

The simplest formula which agrees with these results is $Sb_3 Cl_{13} O$, or three molecules of pentachloride in which two atoms of chlorine are replaced by one of oxygen.

	Calculated.	Found.
Antimony	43·39	43·46
Chlorine	54·71	54·75
Oxygen	1·90	——

That this is a definite compound, and not a mere mixture of pentoxide and pentachloride ($Sb_2 O_5 + 14\ Sb Cl_5$), is evident from the fact that the latter substance is not

dissolved out by washing with carbon disulphide. The calculated percentage of pentoxide contained in this compound is 7·68; on heating 2·517 grms. of the oxychloride in a tube retort, a residue of 0·1799 grm. of pentoxide remained, corresponding to a percentage of 7·14.

The second oxychloride formed by heating the mixture of one molecule of pentoxide and three of pentachloride, is produced only in small quantities, as yellowish crystals. To obtain it in the pure state, that portion of the tube in which the substance is found is cut off, and, after the tube has been resealed, it is placed in a slanting direction in a vessel containing water heated from 85° to 90°. The $Sb_3 Cl_{13} O$ melts and runs down, leaving the other less fusible oxychloride behind; this is then dried on a porous plate *in vacuo* over solid caustic potash. Two determinations showed that the melting-point of this substance is 95°·5 C. On analysis, the following results were obtained :—

(A.) *Antimony-determinations.*

Preparation.	Method.	Weight of substance taken.	Percentage of antimony.
No. 1.	1.	0·8170 gave 0.5534 $Sb_2 O_4$.	53·66
No. 2.	1.	0·4225 gave 0·3245 sulphide, of which 0·2972 gave 0·2656 $Sb_2 O_4$.	54·12

Mean of two antimony-determinations 53·89 Sb.

(B.) *Chlorine-determinations.*

(Chlorine precipitated in filtrate from antimony-determination.)

Preparation.	Weight of substance taken.	Weight of Ag Cl.	Weight of Ag.	Percentage of chlorine.
1.	0·1240	0·1645	0·0135	36·48
1.	0·2385	0·3450	0·0060	36·61
1.	0·2850	0·4050	0·0140	36·77
2.	0·1676	0·2370	0·0008	36·49

Mean of four chlorine-determinations 36·58 Cl.

The simplest formula agreeing with these numbers is $Sb_3 O_4 Cl_7$, or three molecules of antimony pentachloride in which four atoms of oxygen replace eight of chlorine.

	Calculated.	Found.
Antimony	53·94	53·89
Chlorine	36·62	36·58
Oxygen	9·44	——
	100·00	

From the above results it is clear that the simple reaction by which phosphorus oxychloride may be formed is not reproduced under similar circumstances in the antimony series, but that this element, in agreement with its general deportment, gives rise to more complicated compounds.

The oxychlorides derived from antimony trichloride have been frequently examined; the results of the analysis of powder of algaroth made by different investigators vary considerably; and Sabanejeff has recently shown that these differences are probably due to the presence in the substance of antimony trichloride in varying quantities. This impurity he gets rid of by washing the oxychloride, obtained by the action of a large excess of water on the trichloride, with ether or carbon disulphide, in which the trichloride dissolves. In this way he obtains a compound having the constant composition $Sb_4 Cl_2 O_5$, or two molecules of trioxide, in which one atom of oxygen is replaced by two of chlorine; whilst a simpler monoxychloride, Sb O Cl, is prepared by acting with only from 2 to 10 molecules of water on the trichloride; but this, on treatment with ether or carbon disulphide, loses trichloride and yields $Sb_4 Cl_2 O_5$, thus :—

$$5\,Sb\,O\,Cl = Sb\,Cl_3 + Sb_4\,Cl_2\,O_5.$$

The results of my experiments lead me to the conclusion that the body obtained by the action of boiling water on

the trichloride does not possess the composition $Sb_4Cl_2O_5$, but consists of ten molecules of this mixture and one of the trichloride, which latter, however, can be removed by washing with either carbon disulphide or ether. Antimony-determinations in two preparations gave (1) 75·45 per cent. Sb, (2) 75·88 per cent. Sb; corresponding chlorine-determinations gave (1) 12·43 per cent Cl, (2) 12·49 per cent. Cl.

Hence we have

	Calculated for $10\,Sb_4Cl_2O_5 + SbCl_3$.	Calc. for $Sb_4Cl_2O_5$.	Found.
Sb	75·57	76·37	75·66
Cl	12·34	11·11	12·46
O	12·09	12·52	——

By acting upon 15 parts by weight of antimony trichloride with 1 part of trioxide in a sealed tube, Schneider (Pogg. Ann. viii. 407) obtains a crystalline oxychloride, to which he assigns the formula $7\,SbCl_3 . Sb\,O\,Cl$. Repeating Schneider's experiments, I obtained a pearl-grey crystalline mass melting at 72° C., the melting-point of the trichloride. When acted upon by absolute alcohol, it yields powder of algaroth ($Sb_4Cl_2O_5$); and its composition appears to be even more complicated than that assigned to it by Schneider.

Antimony-determinations in two specimens gave (1) 54·24 per cent. Sb, (2) 54·16 per cent. Sb; whilst the corresponding chlorine-estimations were (1) 45·69, (2) 45·87, instead of 55·08 per cent. Sb, and 44·62 per cent. Cl, required by Schneider's formula—but agreeing with the formula $Sb_{16}Cl_{46}O$, which requires 54·2 per cent. of antimony, and 45·35 per cent. of chlorine.

The differences here found between the substances as prepared by Schneider and myself may arise from the admixture of antimony trioxide with the oxychloride in the former preparation. When the tube in which the substance has been prepared is placed in an upright posi-

tion and allowed to cool, the undissolved oxide sinks to the bottom of the tube; but on still further cooling, when the contents of the tube are about to solidify, the oxide rises from the bottom and mixes with the oxychloride. To obtain the substance perfectly free from the undissolved oxide, the contents of the tube are gently heated, and, when the finely divided oxide is deposited, the clear liquid oxychloride is drawn off with a pipette.

IX. *Arsenic in Pyrites and various Products.* By H. A. Smith. Communicated by Professor Roscoe, F.R.S.

Read April 4th, 1871.

The presence of arsenic in the various kinds of pyrites used for the production of sulphuric acid has been a great drawback to manufacturers from its first introduction, and has prevented the use of this mineral to the extent it might otherwise have attained.

The arsenic, difficult to get rid of commercially, passes from the pyrites to the sulphuric acid, from the acid to the products in the manufacture of which the acid has been employed, its presence alone preventing their use in very many cases.

In the analyses I have seen given of pyrites the amount of arsenic present seems very trivial; and, indeed, had the specimens analyzed been fair samples of the general ores supplied to manufacturers, there would be little or no necessity for complaint. The difference existing between the amount of arsenic in pyrites obtained by laboratory analysis and that found in practical working led me to

make the following series of experiments. These enable us also at the same time to see whether the methods of analysis usually employed are sufficiently accurate for the purpose.

The difference between my results and those generally given was so enormous, that it was only after many repetitions that I could believe those now given to be correct. The numbers, however, obtained in the laboratory have been borne out in a great measure by the amounts of arsenic obtained in the manufactured products, the percentage of arsenic in the latter being evidently out of all reasonable proportion to the percentages given in published analyses of the pyrites itself. The results are arranged in three Tables.

In Table I. Part 1 is given the analyses of various specimens of pyrites; these are extracted from Richardson and Watts's 'Technology.' In Part 2 my own analyses are given in full of a few specimens of the same species of ore.

Table II. gives the amount (percentage) of arsenic in a certain kind of pyrites, in the sulphuric acid manufactured from it, and in the various products in the manufacture of which the sulphuric acid has been used. Also the amount of arsenic in the chamber- and flue-deposits.

Table III. is calculated from Table II., giving the amounts in a manner more useful for practical purposes.

In order to obtain a proper sample, a good deal of care was taken to get what might be considered a fair average of the ore.

Forty specimens of the pyrites to be examined were taken from various parts of the lode; these were reduced to a fine powder, intimately mixed and dried. A portion of this was weighed out for analysis; I then concluded that the sample might be considered a very fair average one.

The pyrites used for the analyses given in Table II. was the Hard Norwegian.

Before going further I may mention that the methods of analysis employed were as follows :—

A weighed quantity of the ore was fused with three parts of pure carbonates of soda and potash, and one part pure nitrate of potash, for about ten minutes. The addition of chloride of sodium was, I found, perfectly unnecessary for the estimation of the arsenic. The residue was then boiled well with water and filtered; the insoluble portion was again treated in the same manner, and the filtrate from it added to that of the former. This filtrate was carefully evaporated to one quarter of its bulk, and the arsenic determined by two separate methods,—1st, by precipitation as tersulphide of arsenic; and, 2ndly, as the arseniate of ammonia and magnesia, both dried on a weighed filter.

The methods of analysis of the products obtained from the sulphuric acid were simply modifications of this process, the unnecessary portions being omitted.

The numbers given in Part 1 of Table I. show, I think, a little anomaly at the outset. As in practice we find that the great difficulty a manufacturer has to contend against in burning pyrites is the arsenic, he will naturally prefer the ore with the least objectionable matter present. Here we find four species with only a "trace" of arsenic present, and one with actually "none." I do not think manufacturers, as a rule, would run so much against their own interests as to use a pyrites containing arsenic when they could buy another kind free from it; but, according to this analysis, they must make a great error, as I believe there is a pretty fair demand for both Belgian and Westphalian pyrites, and that even Spanish finds ready customers, although the Norwegian is said to be free from arsenic impurity. I am afraid we must lay this fact not altogether to the stupidity of the manufacturers, but partly

Table I.

Part 1.		Part 2.	
Name of Pyrites.	Arsenic per cent. Mean.	Arsenic per cent.	Mean.
Spanish	0·21 to 0·31	Spanish—	
Belgian	Trace.	Tharsis' ..... 1·542, 1·620, 1·644, 1·790, 1·526, 1·700, 1·552, 1·661, 1·686, 1·796 ...	1·6517
Westphalian	Trace.	Mason's ..... 1·744, 1·810, 1·891, 1·770, 1·661, 1·692, 1·711, 1·754, 1·601, 1·719 ...	1·7453
Norwegian	None.	Belgian ..... 1·000, 0·664, 1·621, 0·743, 1·002, 0·624, 0·972, 0·924.....	0·9437
Irish	0·33	Westphalian ..... 1·886, 1·794, 1·802, 1·936, 1·899, 1·900, 1·921, 1·889.....	1·8783
Cornish	0·32	Norwegian—	
Italian	Trace.	Hard ..... 1·440, 1·916, 1·633, 1·621, 1·648, 1·611, 1·692, 1·631.....	1·6490
Swedish	Trace.	Soft ..... 1·794, 1·731, 1·664, 1·632, 1·700, 1·621, 1·816, 1·709.....	1·7085
Cleveland	——		

to the fault of the analysts. It was with this idea that I worked out Part 2 of the same Table, which I think somewhat explains the fact.

The difference in the numbers found is certainly large; but, as I said before, I find them borne out in a great measure by other analyses which I will shortly mention.

The purest pyrites is the Belgian, containing only 0·9437 per cent. As; but this carried along with it the very great inconvenience (which also became an expense) that, in breaking for the kilns, a large amount of "smalls" accumulated; and it was therefore put aside so as to make room for that containing the next smallest amount of arsenic, the Hard Norwegian, which had the great advantage of making little or no "smalls." This contained a decidedly larger amount of arsenic than the last, namely 1·6490 per cent. As. The increase found by laboratory analysis was also corroborated by the practical working.

In Table II. I have endeavoured to show how this amount of arsenic is distributed among the separate products of manufacture.

After this pyrites (Norwegian), containing originally 1·649 per cent. As, has been burnt in the kilns, we find the amount reduced to 0·465 per cent. As, leaving thus 1·184 per cent. As to be accounted for. As is naturally expected, the greatest quantity of this finds its way into the sulphuric acid, the amount there present being 1·051 per cent. As; so that 0·133 per cent. As has been lost between the kilns and the chamber.

In a flue leading from the kilns to the lead chamber a yellowish white, partly crystalline, deposit was found, consisting of arsenious and arsenic acids mixed with sulphur in a viscous state. This, on analysis, gave 46·360 per cent. of arsenic. Unluckily this number becomes of very little use to us here, as this was a flue that had been in use for a very long time; so that we have an

Table II.

Substances in which Arsenic is present.	Arsenic (Trioxide) per cent.															Mean.
Pyrites (Hard Norwegian)—																
Before burning	1·440	1·916	1·633	1·621	1·648	1·611	1·692	1·631	.....	.....	.....	.....	.....	.....	.....	1·649
After burning	0·562	0·356	0·469	0·454	.. ...	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	0·465
Sulphuric acid	1·112	1·026	1·051	1·190	1·141	0·991	0·861	1·036	1·100	1·420	1·003	1·032	0·800	1·006	0·999	1·051
Deposit in flue leading from pyrites-kilns to lead chamber.....	46·499	46·431	45·911	46·600	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	46·360
Deposit on bottom of lead chamber.....	1·861	1·900	1·811	1·825	1·890	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	1·857
Hydrochloric acid	0·711	0·642	0·686	0·599	0·700	0·693	0·589	0·911	.....	.....	.....	.....	.....	.....	.....	0·691
Sulphate of soda	0·006	0·044	0·050	0·032	0·024	0·020	0·023	0·021	0·033	0·045	0·009	.....	0·060	0·081	.....	0·029
Soda waste.....	0·611	0·222	0·190	0·462	0·527	0·643	.....	.....	.....	.....	.....	.....	.....	.....	.....	0·442
Carbonate of soda.....	——	——	——	——	——	——	——	——	——	——	——	——	——	——	——	——
Recovered sulphur—																
Mond's process.....	0·901	0·842	0·616	0·442	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	0·700
After purification...	——	——	——	——	——	——	——	——	——	——	——	——	——	——	——	——

accumulated deposit from which we can procure no relative numbers.

In the chamber itself we find another deposit having here and there beautifully regular crystals, mostly showing transparent elongated prisms, evidently of arsenic acid. On analysis this deposit gave 1·857 per cent. of As, there being present besides large quantities of silica and other insoluble impurities. I am sorry that the observation applied to the previous deposit must also be applied to this, that it has accumulated too long for any useful numbers to be got from it. However, this shows us so far where the amount of Arsenic missing in the Sulphuric acid has been collected.

In regard to the products in the manufacture of which Sulphuric acid has been used, we come first to Hydrochloric acid. Of eight analyses, the mean obtained is 0·691 per cent. As. Considering the amount in the Sulphuric acid, this number is comparatively small. I have not been able to examine any of the coke in the condensing-towers; but I there expect to find a good deal of arsenic collected, as we find only 0·029 per cent. As left in the sulphate of soda. I have also made a few analyses for arsenic of the gas escaping to the chimney from the reverberatory furnace; but the numbers I do not give at present, hoping to have a more extended series shortly.

In some samples of soda waste, the production of which I did not see, there was found nearly half a per cent. of arsenic, or 0·442 per cent. This, however, can explain nothing relating to the former numbers, as it came from different manufactories. Still it shows how persistently arsenic sticks to the products, and how difficult it is to get rid of it.

In twelve analyses of carbonate of soda got from six different places of manufacture I was unable to find a trace of arsenic; still I do not feel at all confident on this head, and

expect that I may still come across some specimens containing at least a "trace."

Some samples of Sulphur recovered by Mond's process gave a very large amount of arsenic, 0·7002 per cent.; but this was before it had undergone the process for purification, as I have since had some prefectly free and pure.

TABLE III.

Tons.					Ton As.
100	Hard Norwegian pyrites (Table I.) contain, before burning				1·649
,,	,,	,,	,,	after burning	0·465
		Tons.			
100	Hard Norwegian pyrites make	140·875	H_2SO_4,	containing	1·481
140·875	Sulphuric acid make	104·9	HCl	,,	0·724
,,	,, ,,	204·12	Na_2SO_4	,,	0·059

In this Table I have given the numbers on a practical scale, so that one may see the amount of impurity we have to deal with. Viewed in this way it looks really serious. In every 100 tons of pyrites we have $1\frac{1}{2}$ ton of arsenic, and nearly half a ton left in the burnt residue. The difference, or 1 ton, spreads itself through the whole process of soda-making.

If the arsenic is to be expelled at all, it must be expelled from the sulphuric acid. In some places this is already in operation; but this process always carries with it the other great inconvenience, not only to ourselves but to our neighbours, of the disagreeable smell of sulphuretted hydrogen. In some places in Germany, in the Hartz &c., I believe they clear the acid by simply passing this gas through it; it is, however, difficult to imagine how this can possibly be remunerative. However, this is one of the many open questions in alkali manufacture the working out of which will be a gain to the manufacturer.

Arsenic is not only an expensive impurity to the manufacturer, extending itself to the products, but it also acts upon the nitric acid in the lead-chamber.

It is a common thing in Sulphuric-acid manufacture to find the acid which has passed through the Gay-Lussac tower of a deep red colour. This has been referred to the presence of the lower oxides of nitrogen, their presence being explained by the fact that the mixture of nitrate of soda and sulphuric acid, used for the production of nitric acid, has been heated to too high a temperature; this may be one good reason for it, but I think the arsenic must also come in for a share of the blame. The arsenic is expelled from the pyrites as arseni*ous* acid, and, coming in contact with the nitric-acid gas, is oxidized and converted into arsen*ic* acid, always causing, of course, a loss of nitric acid, and in part a conversion of it into the lower oxides of nitrogen. That such happens I have no doubt, as in the deposit formed in the chamber I found clear and perfect crystals of arsenic acid.

But this seems like getting up a case of prosecution against arsenic, and enumerating and multiplying the evils manufacturers have to fight against. Whether greater evils be found out or not, they have their work before them in discovering methods of getting rid of their persistent enemy.

X. *A Study of certain Tungsten Compounds.* By Henry E. Roscoe, B.A., Ph.D., F.R.S., Professor of Chemistry in Owens College, Manchester.

Read January 23rd, 1872.

The formulæ of the tungsten compounds, the atomic weight of the metal, and even its elementary nature are subjects upon which, for many years, serious doubts have

been expressed. Thus, Persoz (Ann. Ch. Phys. [4] i. p. 93) comes to the conclusion that the atomic weight of tungsten and the formula of its highest oxide are not 184 and WO_3 respectively, as generally adopted, but that the metal is to be considered as belonging to the arsenic group, having an atomic weight of 153, and forming a pentoxide and a pentachloride known as the tungstic series, together with a lower series of compounds which correspond to the lower arsenic compounds*. This supposition of Persoz's, whilst unsupported by sufficient experimental evidence of its own to attract much attention from chemists, and contradicted by the important fact of the normal atomic heat of the metal corresponding to the old atomic weight, has never been satisfactorily proved to be incorrect, and has received a certain amount of corroboration from the subsequent vapour-density determinations of the chloride of tungsten published by Debray (Compt. Rend. lx. p. 820, 1865). In this research Debray shows that the vapour-density of tungstic chloride, taken in mercury and sulphur vapours, is 168·5 (H=1), the normal density for WCl_6 (W=184) being 198·5, whereas that for Persoz's tungstic chloride, $TuCl_5$ (Tu=153), is 165, and therefore closely corresponds to the experimental density.

In order to clear up these questions, a thorough investigation of the chlorides and oxychlorides of tungsten, together with the corresponding bromine and iodine compounds, appeared before all things necessary.

Preparation of Metallic Tungsten.—Tungstic acid was first prepared by decomposing 2 kilogs. of the commercially pure tungstate of soda with hydrochloric acid. The precipitated tungstic acid was repeatedly washed by decan-

* At a later date (Bull. Soc. Chimique, 1864, ii. p. 188) MM. Persoz (père et fils) even state that they have succeeded in separating from the crude tungstates several radicals, giving rise to acids possessing different properties and different compositions.

tation, the mass being heated by steam, and the rapid settling of the precipitate being brought about by the plentiful addition of sal-ammoniac. After complete washing, the acid, drained on a Bunsen's filter, was mixed with excess of strong ammonia, which converted it completely into white ammonium salt. This was then dissolved in water, and the solution filtered and concentrated, when several crops of needle-shaped crystals were obtained. Care must be taken in the last crystallization to prevent (by the addition of strong ammonia) the formation of flat scales of a double tungstate of sodium and ammonium, which yields, on heating, an impure greenish acid.

The crystals of ammonium tungstate were then converted into tungstic anhydride by strong ignition in access of air. This oxide possesses a bright canary-yellow colour, and becomes dark orange on heating. A very slight admixture of sodium-salt imparts to the oxide a greenish tint, which no amount of oxidation can remove; it also becomes greenish on exposure to light.

The metal was prepared by igniting the tungstic anhydride, contained either in a platinum or in a porcelain tube, to bright redness in a current of dry and purified hydrogen. The metal powder thus prepared possessed a light grey bright metallic lustre; on oxidation it gained 26·1 per cent., that required by W = 184 to WO_3 being 26·09 per cent.

Three determinations of the specific gravity of the metal, prepared on two different occasions, gave:—

Preparation A, specific gravity in water at			12°·0	19·261
„	„	„	18°·0	19·213
Preparation B,	„	„	9°·5	18·950

or mean specific gravity, referred to water at 4°, 19·129. This number is considerably higher than that found by previous observers.

The Chlorides of Tungsten.

1. *Tungsten Hexachloride,* WCl_6.—In the preparation of this chloride in the pure state it is absolutely necessary to exclude every trace of oxygen or of moisture. For this purpose the pure metal must be burnt in a current of perfectly dry and air-free chlorine, otherwise some red oxychloride is invariably formed; and this cannot be separated from the chloride, owing to the slight difference existing between their boiling-points.

Metallic tungsten takes fire at a moderate heat in dry chlorine, and the action goes on by itself until all the chlorine disappears. In order to obtain the hexachloride in quantity, the metal was first ignited in a current of dry hydrogen, then the hydrogen in the whole apparatus was displaced by a current of dry carbonic-acid gas, and, lastly, air-free chlorine was substituted for the carbonic acid, and the tube or retort heated moderately. At the commencement of the operation a slight sublimate of red needle-shaped crystals of the oxychloride is frequently formed, owing to the unavoidable presence of traces of oxygen; but this is easily driven to the end of the tube beyond the point at which it is intended to collect the hexachloride. On raising the temperature of the metal, a granular sublimate of dark violet opaque crystals of the hexachloride makes its appearance. Prepared in quantity, the hexachloride collects as a dark blackish-red liquid. In order to saturate this liquid, it was slowly distilled in a current of chlorine, by which means any traces of red oxychloride were got rid of.

The dark violet crystals decrepitate on cooling, and the mass can thus be easily transferred to the tube in which it is sealed up. When pure, the solid hexachloride does not undergo any change even in moist air; but in presence of the slightest trace of the red oxychloride it at once absorbs

moisture, evolving copious fumes of hydrochloric acid, and the colour changing from a violet to a greenish brown. Water does not act upon pure hexachloride of tungsten in the cold; but, on boiling, the decomposition into tungstic acid and hydrochloric acid occurs. If, however, oxychloride is present, the red needle-shaped crystals at once become yellow, or, if an impure mass is employed, the whole is suddenly decomposed by cold water into a greenish oxide. Tungsten hexachloride dissolves readily in dry carbon disulphide, forming a dark brown solution, from which the substance is deposited in six-sided plates; but, as the oxychloride is likewise soluble in this liquid, this mode of preparation was not adopted. One of the tubes in which the crystals thus obtained had been sealed, exploded with a loud report on opening with a file, the crystals suddenly assuming the condition of the decrepitated chloride. On decomposing the hexachloride with water, a small quantity of chlorine is invariably retained by the tungstic acid even on repeated distillation with water; and, in order to obtain the whole of the chlorine, it was found necessary to reduce the oxide to metal. The analysis was therefore carried out as follows:—the pure chloride was weighed in a porcelain boat, then carefully covered with water, and the boat and contents quickly introduced into a combustion-tube already fitted to a gas-furnace. A current of purified hydrogen was delivered at one end of the combustion-tube, whilst the other was connected with a bent tube dipping into a flask containing water for the absorption of the hydrochloric acid. On gently heating the fore part of the tube (the greatest care being necessary to avoid spirting), the chloride is converted into the yellow oxide, which, after being perfectly dried, is gradually heated to as high a temperature as the tube will stand. The tungsten is thus obtained in the metallic state and the chlorine as hydrochloric acid, which is estimated with silver.

The following six analyses were made from six different preparations :—

Analyses I. and II. Substance distilled three times in chlorine over charcoal.

Analysis III. A large quantity of chloride was boiled in a retort whose neck dipped under mercury until no more red oxychloride could be seen to come over. Still this residue contained a trace of lower chlorides, as it rapidly decomposed on addition of cold water.

Analysis IV. No. III. was twice redistilled in chlorine and again treated in a retort as before. The substance still contained traces of lower chloride, as it was decomposed by cold water.

Analysis V. Prepared by burning pure metal in dry chlorine and redistilling.

Analysis VI. Residual chloride left from a vapour-density experiment of the material No. IV.

Analyses of Tungsten Hexachloride, WCl_6.

	I.	II.	III.
Weight of chloride taken	2·1974	1·5719	2·0284
„ tungsten obtained	1·0117	0·7321	0·9508
„ silver chloride	4·7654	3·3941	4·3467
Percentage of tungsten	46·04	46·57	46·87
„ chlorine	53·64	53·42	53·01
	99·68	99·99	99·88

	IV.		V.		VI.
Weight of chloride taken	1·4185	1·3744	0·8443	1·6678	0·9210
„ tungsten trioxide	—	0·8036	—	0·9757	0·5423
„ silver chloride	3·0540	—	1·8220	—	1·9823
Percentage of tungsten	—	46·37	—	46·39	46·69
„ chlorine	53·26	—	53·39	—	53·22
	99·63		99·78		99·91

		Calculated.	Mean found.
Tungsten, W	=184	46·35	46·40
Chlorine, Cl_6	=213	53·65	53·32
	397	100·00	99·81

The exact determination of the melting-point of the hexachloride is attended with some difficulty, as the liquefaction takes place gradually, and the smallest trace of oxychloride depresses the melting-point down to about 180° C., that given by the older observers (180° Forcher, 183° Cahours). A mean of several determinations of the pure substance gave the number 275° C. (corrected) as the true melting-point, and 270° (corrected) as the point of solidification. The constant boiling-point of the liquid was found to be 346°·7 (corrected) under a barometric pressure of 759·5 millims.

The vapour-density of tungsten hexachloride was determined (1) in sulphur-vapour at 440°, and (2) in mercury-vapour at 350°. As the hexachloride always leaves, on distillation, a small quantity of solid residue, the substance was distilled (either in a current of carbonic acid or of chlorine) into the heated bulb from a smaller one attached to it, according to the method employed by me in the determination of vapour-density of vanadium tetrachloride. The narrow neck of the bulb was kept open by inserting a platinum wire; and, after the sulphur or mercury had been boiling for some minutes, the neck was sealed.

Determinations of Vapour-density of WCl_6 *in Sulphur-vapour at* 440°.

	I.	II.	III.
Weight of bulb and air	22·7797	14·5806	21·8781 grms.
Barometer	755·7	755·4	755·4
Temperature	14°	14°	12°
Weight of bulb and vapour, sealed at 440° C.	23·6605	14·9652	22·3135 grms.
Barometer	761·0	755·4	755·4
Capacity of bulb	193·63	84·25	97·36
Residual air*	0·20 c.c.†	0·42 c.c.‡	0·94 c.c.‡
Hence vapour-density (H=1)	167·8	169·7	168·8

* Coefficient of expansion of glass, 0·00003.

† At 16° and 633 millims. ‡ At 14° and 640 millims.

Giving a mean of 168·8 as the density for 440° C.; whilst the calculated density is 198·5. This closely corresponds to Debray's experimental number of 170.

Determination of Vapour-density of WCl_6 *in Mercury-vapour at* 350°.

As there is only a difference of 3° between the boiling-points of the hexachloride and of mercury, a condensation of the substance in the neck of the bulb may possibly have occurred, although there was no indication of this having happened. In a third experiment the number 199 was obtained; but in this case a distinct residue was noticed, the chloride not having been redistilled in the usual way from the small bulb, and this determination is therefore omitted.

	I.	II.
Weight of bulb and air	23·9960	13·6360 grms.
Barometer	758·5	756
Temperature	20°	20°
Weight of bulb and vapour, at 350° C.	25·2040	14·1525 grms.
Barometer	758·5	756
Capacity of bulb*	193·2	82·7
Hence vapour-density (H=1)	190·7	191·2

Giving a mean vapour-density of 190·9 at 350°, instead of 198·5 as calculated from the formula.

The fact of the alteration of the vapour-density from 190 at 350°, or only 3° above the boiling-point, to 167 at 440°, shows pretty clearly that the anomalous vapour-density is to be ascribed rather to dissociation than explained by Persoz's suggestion of an error existing in the atomic weight of the metal, according to which the atomic weight is five-sixths of 184, and the chloride is supposed to be $TuCl_5$, requiring a vapour-density of 165·4.

* Expansion of glass not allowed for.

This conclusion is fully borne out by further experiments detailed in the sequel*.

The residual hexachloride remaining in the bulb exhibits the same properties, and analysis shows that it possesses the same composition (see Analysis No. VI.), as the original substance; and there is no trace of free chlorine found in the cold bulb, nor does the vapour of the WCl_6 appear to change its peculiar dark red colour at any higher temperature to which it was exposed even when mixed with chlorine or carbonic acid. On treating the residue with water, a difference between its behaviour and that of the original hexachloride could, however, be detected, as it yielded an oxide which was not perfectly yellow, but had a greenish colour, showing the existence of traces of oxides lower than WO_3, although present in too small quantities to affect the results of the analysis.

In order to ascertain whether the gaseous hexachloride is decomposed at high temperatures, a portion of this chloride was distilled upwards for several hours in a current of dry carbonic acid. A continuous liberation of chlorine was clearly shown to occur by passing the exit gas through a solution of potassium iodide from which considerable quantities of iodine were set free. The residual chloride was tested for lower chlorides of tungsten by titrating a weighed quantity with standard permanganate, which readily oxidizes the blue oxide formed by the action of water on the pentachloride into tungstic acid ($W_2O_5 + O = 2WO_3$). 1·2237 grm. of the residual choride decolorized 0·7 cub. centim. of a standard solution, of which 1 cub. centim. corresponds to 0·001278 grm. oxygen. This quantity is required for the conversion of 0·0404

* R. Rieth (Ber. Deutsch. Chem. Gesellschaft, 1870, p. 666) determined the vapour-density of "Wolfram chlorid," showing that it contained 187 of metal instead of 184. There is nothing to prove whether the body thus examined was the hexa- or pentachloride.

grm. of WCl_5; so that the residual chloride contained 3·3 per cent. of pentachloride. In a second experiment about 6 grms. of the hexachloride were distilled upwards in a current of carbonic acid for eighteen hours; 3·9763 grms. of this residue were oxidized with water, sulphuric acid, and permanganate, when 17 cub. centim. of the standard solution were needed. This corresponds to 0·0217 grm. oxygen, or to 0·9806 grm. of WCl_5 oxidized, or the residue contained 24·6 per cent. of the pentachloride. The pentachloride treated in a similar way yields no free chlorine, and therefore does not undergo a similar decomposition at high temperatures.

2. *Tungsten Pentachloride,* $WCl_5 = 361·5$.—On distilling the hexachloride in a current of hydrogen, a reduction always takes place. If the temperature is kept but little above the boiling-point of the hexachloride the dark red colour of the vapour is seen to vanish, and a light greenish-yellow coloured vapour makes its appearance, which soon condenses either to black drops or long shining black needles. After two or three distillations in hydrogen, a pure volatile product is obtained. It is, however, more convenient to reduce at a higher temperature; and in this case the loss of chlorine goes further than one atom, and the distillation is repeated in hydrogen until a solid residue of a lower non-volatile chloride remains, showing that all the hexachloride has been decomposed. The mixture of chlorides thus obtained is then redistilled in a current of dry carbonic acid, when pure pentachloride comes over.

Tungsten pentachloride crystallizes in long, black, shining crystals; if condensed in fine powder its colour is dark green, and the crystals also yield a green powder like potassium manganate when finely divided. The pentachloride is exceedingly hygroscopic, the solid becoming instantly covered with a dark golden-green film on exposure

to air, and small particles being immediately converted into drops. The crystals do not decrepitate on cooling, like those of the hexachloride. On treatment with larger quantities of water the pentachloride forms an olive-green solution, although the greater part is at once decomposed into the blue oxide and hydrochloric acid. With carbon disulphide a deep blue solution is formed; but the chloride is only slightly soluble.

The analyses were made as described under the last compound, except that the water needed for the decomposition was placed in a separate boat, from which it was distilled on to the one containing the chloride in a current of hydrogen; this was necessary, owing to the violent action of water upon the pentachloride.

Analyses of Tungsten Pentachloride, WCl_5.

	I.	II.	III.
Weight of chloride taken	0·8973	1·0781	0·7007
,, tungsten obtained...	——	0·5488	——
,, tungsten trioxide ...	0·5758	——	——
,, silver chloride	——	2·1262	1·3831
Percentage of chlorine	——	48·78	48·38
,, tungsten	50·89	50·90	

	Calculated.	Mean found.
Tungsten, W = 184	50·89	50·90
Chlorine, Cl_5 = 177·5	49·11	48·58
	361·5 100·00	99·48

Tungsten pentachloride melts completely at 248° and solidifies (as a mean of several determinations) at 242° C. (both corrected); and it boils at 275°·6.

The vapour-density of tungsten pentachloride was determined in sulphur-vapour at 440°, and in mercury-vapour at 350°.

Vapour-density of WCl_5 *at* 440° C.

	I.	II.	III.
Weight of bulb in air	19·4245	19·5200	19·6395 grms.
Barometer	772	770	769 mm.
Temperature	14°	12°	12° C.
Weight of bulb and vapour, at 350° C.	19·9790	20·0100	20·3505 grms.
Capacity of bulb*	106·02	93·76	110·58 c. c.
Hence vapour-density (H=1)	186·4	186·5	185·7

Vapour-density of WCl_5 *at* 350° C.

	I.	II.
Weight of bulb in air	25·7517	25·8148 grms.
Barometer	755·5	755·5 mm.
Temperature	13°·5	13°·5 C.
Weight of bulb and vapour...	26·5370	26·3300 grms.
Capacity of bulb	134·3	90·69 c. c.
Hence vapour-density (H=1)	179·9	175·6

The formula WCl_5 requires a density of 180·7; hence the density in the case of this compound is normal, the mean experimental number being 182·8.

3. *Tungsten Tetrachloride,* $WCl_4 = 326$.—The tetrachloride forms the non-volatile residue formed in the distillation of the hexachloride in hydrogen. In order to obtain it in a pure state, the mixture of the two higher chlorides is distilled at a low temperature (best in a bath of sulphuric acid) in a current of dry hydrogen or carbonic acid, and the volatile pentachloride distilled back again once or twice over the residue to saturate the lower chlorides or metal, which are always formed, up to the tetrachloride.

The tetrachloride of tungsten is a loose, soft, crystalline powder of a greyish-brown colour. It is highly hygroscopic, but not so much as the pentachloride, and it is partially decomposed by cold water into brown oxide and hydrochloric acid, and also forms a greenish-brown solu-

* The residual air was not measured; expansion of glass not allowed for.

tion, which is rather more stable than the green solution of the pentachloride in water, but which in course of time deposits insoluble oxide. On evaporating the solution *in vacuo* traces of a crystalline body were obtained. The tetrachloride is non-volatile and infusible under ordinary pressure; but it is decomposed on heating into pentachloride, which distils off, and a lower dichloride, which remains behind. On heating in hydrogen at a temperature above the melting-point of zinc the tetrachloride is reduced to metallic tungsten, some of which is deposited as a black tinder-like powder, undergoing spontaneous ignition on exposure to the air.

Analyses of Tungsten Tetrachloride, WCl_4.

	I.	II.	III.	IV.
Weight of chloride taken ...	1·0100	0·6402	0·4830	1·0804 grms.
„ tungsten obtained	0·5715	——	0·2780	——
„ tungsten trioxide	——	0·4632	——	0·7845
„ silver chloride ...	——	——	0·8344	1·8239
Percentage of tungsten	56·58	57·38	57·55	57·58
„ chlorine	——	——	42·73	41·76

	Calculated.	Mean found.
Tungsten, W = 184.....	56·45	57·22
Chlorine, Cl_4 = 142.....	43·55	42·24
326	100·00	99·46

The want of exact agreement with the calculated numbers probably arises from the facts (1) that, in the first reduction of WCl_6, some metal or the lowest chloride is formed which is not entirely reconverted into WCl_5, and (2) that if the temperature used for the reduction of the pentachloride be a little too high, some dichloride is formed.

4. *Tungsten Dichloride,* $WCl_2 = 255$.—This body is obtained in light grey crusts by reducing the hexachloride at high temperatures in hydrogen. It can be best prepared

by heating either the non-volatile residue or the pure tetrachloride in a current of carbonic acid. For this purpose the temperature of a moderately hot zinc bath was found convenient; if a higher temperature be used metallic tungsten is produced.

The dichloride is a non-volatile loose grey powder without lustre or crystalline structure. It alters perceptibly on short exposure to the air, and dissolves slightly in water, forming a brown solution; the remainder is converted into brown oxide, a slow evolution of hydrogen taking place.

Analyses of Tungsten Dichloride, WCl_2.

Weight of chloride taken	0·7553	1·7258 grms.
„ tungsten obtained ...	0·5590	1·2419
„ silver chloride	0·7595	1·9429
Percentage of tungsten	74·04	71·96
„ chlorine	24·86	27·84

	Calculated.	Found.
Tungsten, W = 184.....	72·15	73·00
Chlorine, Cl_2 = 71.....	27·85	25·35
255	100·00	98·35

For analysis the dichloride was placed in a boat and converted into WO_3 by passing hydrogen saturated with nitrous fumes over it. In this process the formation of some quantity of a white sublimate was observed, which on examination proved to be ammonia salt formed by the reducing action of the dichloride on nitric acid, thus:—

$$W_2Cl_4 + HNO_3 + 3H_2O = W_2O_6 + 4HCl + NH_3.$$

Experiments made in the endeavour to prepare the chlorides WCl_3 and WCl were unsuccessful.

5. *Tungsten Oxychlorides.*—The monoxychloride, WCl_4O, and the dioxychloride, WCl_2O_2, have been already tolerably fully studied by Bloomstrand and Riche; nevertheless we find that Persoz doubts the existence of these well-charac-

terized compounds*, and Debray, obtaining abnormal numbers for the vapour-density of the monoxychloride, is unable to explain the cause of his results.

The splendid, red, needle-shaped crystals of the monoxychloride are best obtained by passing the vapour of a chloride over heated oxide, or dioxychloride, in a current of chlorine. The crystals melt at 210°·4 (corrected), and solidify at 206°·7 C.; when heated more strongly, the liquid boils at 227°·5 (corrected), forming a red vapour, rather lighter coloured than that of the hexachloride. On repeated distillation over red-hot charcoal in a current of chlorine the hexachloride is formed. On exposure to air it becomes at once covered with a yellow crust of tungstic acid, from which it is difficult to obtain it quite free.

Analysis gave:—

	Calculated.	Found.
Tungsten, W	53·80	53·89
Chlorine, Cl_4	41·52	41·11
Oxygen, O	4·68	——

Debray (Compt. Rend. lx. p. 820) determined the vapour-density of the monoxychloride in sulphur-vapour to be 148 (H = 1), whereas the calculated density is 171. It appeared therefore of interest to repeat this determination.

Vapour-density of WCl_4O *in Sulphur-vapour.*

	I.	II.
Weight of bulb in air	21·6315	20·5745 grms.
Barometer	754	761 mm.
Temperature	11°	13° C.
Weight of bulb and vapour, at 440° C.	22·2965	21·0780 grms.
Capacity of bulb	142·73	107·35 c.c.
Hence vapour-density (H = 1)	171·3	171·7

* "Le tungstène, pas plus que le phosphore, n'engendre d'oxychlorure. Les composés que l'on a ainsi désignés sont des combinaisons en proportions définies, mais variables, d'acide anhydre avec le chloride correspondant."—*Comptes Rendus*, lvii. p. 766.

Vapour-density of WCl_4O *in Mercury-vapour.*

	I.	II.
Weight of bulb in air	22·6345	24·1850 grms.
Barometer	764	767 mm.
Temperature	10°	10° C.
Weight of bulb and vapour, at 350° C.	23·1415	24·7745 grms.
Capacity of bulb	89·45	107·7 c. c.
Hence vapour-density (H = 1)	175·8	170·2

Hence it appears that the vapour-density of tungsten monoxychloride has the normal density.

The Dioxychloride, WCl_2O_2, is best prepared by passing chlorine over the brown dioxide. Combination takes place at a moderate heat, the oxide becoming covered with a whitish crust, which, on increase of temperature, distils off without melting, condensing in small square scales of a whitish-yellow colour. The dioxychloride volatilizes at a temperature near to a red heat, with partial decomposition. The crystals are infusible, and are not acted upon by moist air or cold water. Even when boiled with water the dioxychloride is not completely decomposed, the unaltered substance being volatilized on evaporation; and in the analysis, in which moist hydrogen is passed over the substance, great care has to be taken to prevent loss by volatilization of the compound. If the metal only is to be determined, the oxychloride may be dissolved in ammonia, and the solid mass ignited when WO_3 is left.

The following were the results of the analyses :—

	Calculated.	Found.
Tungsten	64·32	64·11
Chlorine	24·31	24·74
Oxygen	11·37	——
	100·00	

The vapour-density of the dioxychloride could not be determined, as at 440° C. the contents of the bulb remained liquid.

Bromides of Tungsten.

Bromine-vapour, or, more conveniently, carbonic acid saturated with bromine, acts rapidly on red-hot tungsten, forming dark bromine-like vapours, which condense to a crystalline sublimate. Especial precautions, similar to those necessary in the preparation of the chlorides, must be employed with the bromides, as the oxybromide formed in presence of air or moisture possesses very nearly the same colour as the bromide, and therefore the detection of an impurity is not so easy as in the case of the chloride.

1. *Tungsten Pentabromide,* $WBr_5 = 584$.—By the action of excess of bromine on tungsten, a penta- and not a hexabromide is obtained; indeed the pentabromide itself evolves bromine on standing, showing that it is an unstable compound. Thus prepared, and distilled in excess of bromine, the pentabromide forms dark crystals having a metallic lustre, not unlike those of iodine; these melt at 276° C. (corrected), and solidify at 273°, and the substance boils at 333°. The pentabromide is at once decomposed by excess of water into the blue oxide of tungsten and hydrobromic acid, and immediately undergoes decomposition on exposure to moist air. On distillation, a small quantity of a lower non-volatile bromide remains behind; and this explains the slightly too high percentage of metal found in the analysis.

The body employed in Analysis I. was obtained directly by the action of bromine on the metal; for Analysis No. II. the same body was employed after it had been heated for eighteen hours at 100° with excess of bromine, and this distilled off at a low temperature. The analyses were conducted as described for the chlorides, except that the hydrobromic acid and free bromine evolved were passed into a solution of sulphurous acid.

	I.	II.
Weight of bromide taken	1·3750	1·6200
„ tungsten obtained ...	0·4570	0·5144
„ silver bromide	2·0956	2·633
„ metallic silver	0·0129	0·0172
Percentage of tungsten	33·24	31·75
„ bromine	65·56	69·93

	Calculated.	Found.
Tungsten, W = 184	31·51	32·49
Bromine, Br_5 = 400	68·49	67·74
	584 100·00	100·23

Reduction of Pentabromide in Hydrogen.—When pentabromide is strongly heated in a current of hydrogen, the metal is reduced in the form of a fine black powder, which takes fire spontaneously on exposure to air; so that the complete reduction of the bromides takes place much more readily than in the case of the corresponding chlorides.

If the pentabromide be treated with hydrogen at 350° for several hours, the resulting substance (which is not homogeneous) consists of hard black crusts covered with a velvety substance. It is scarcely acted upon by water, and on analysis gave the following percentages:—

	I.	II.	III.
Percentage of tungsten	43·53	44·48	45·35
„ bromine	——	——	50·30

The percentage of tungsten agrees nearly with that needed for the formula WBr_3; but that of the bromide is too low, owing to admixture of oxybromide.

2. *Tungsten Dibromide,* $WBr_2 = 344$.—This mixture, when heated in a bath of fused zinc-chloride in a current of hydrogen, gave off a large quantity of volatile bromide and oxybromide, leaving a residue of a bluish-black velvety colour. This latter substance is WBr_2, tungsten dibromide. The dibromide is decomposed by water, forming a brown solution with evolution of hydrogen. Acted on by nitric acid, tungsten trioxide is formed and bromine

evolved; and when the vapour of the same acid is passed over the heated body, the formation of an ammonium salt was observed as in the case of the dichloride.

Analysis No. I. was made from a portion of substance which was partially oxidized, Nos. II. and III. from a purer preparation.

Analyses of Tungsten Dibromide, WBr_2.

	I.	II.	III.
Weight of substance taken	0·6354	1·1874	0·8040
„ tungsten obtained ...	0·3261	0·6149	——
„ tungsten trioxide ...	——	——	0·5374
„ silver bromide	0·6287	1·3112	——
„ silver	0·0260	0·0068	——
Percentage of tungsten	51·32	51·78	53·01
„ bromine.....	45·12	47·41	——

	Calculated.	Mean.
Tungsten, W = 184	53·49	52·03
Bromine, Br_2 = 160	46·51	46·26
	100·00	98·29
344		

Oxybromides of Tungsten.

1. *Tungsten Monoxybromide,* WBr_4O.—This substance is formed, together with the dioxybromide, by acting on a heated mixture of equal parts of metal and tungsten dioxide by bromine. It forms in brownish-black, crystalline, shining needles, which are easily fusible, and can be separated from the dioxybromide by gentle sublimation, when the latter compound remains behind. The monoxybromide melts at 277°, and boils at 327°·5 (corrected), and is readily acted on by water.

The preparation used for Analyses I. and II. contained a small quantity of dioxybromide.

	I.	II.	III.	IV.
Weight of substance taken ...	0·9980	1·1985	1·2218	1·7388
„ tungsten found ...	0·3665	0·4419	——	——
„ tungsten trioxide...	——	——	0·5624	——
„ silver bromide.....	——	1·6683	——	2·5041
„ silver	——	0·0163	——	0·0130

	I.	II.	III.	IV.
Percentage of tungsten	36·72	36·87	36·50	——
„ bromine	——	60·25	——	61·83

	Calculated.	Found.
Tungsten, W = 184	35·38	36·69
Bromine, Br_4 = 320	61·54	61·04
Oxygen, O = 16	3·08	——
520	100·00	

2. *Tungsten Dioxybromide,* WBr_2O_2.—This compound is formed on passing bromine-vapour over red-hot tungsten dioxide, or (together with the monoxybromide) by passing the vapour of the pentabromide over tungsten trioxide. The light, reddish-brown coloured vapours of dioxybromide easily condense to dark, reddish-brown coloured, well-formed crystals and crystalline crusts, which, on cooling, form light red, transparent crystals, yielding a yellow powder. The crystals do not melt, but volatilize at a temperature near to a red heat. On resublimation, a partial decomposition occurs, a small quantity of tungstic oxide remaining, and a sublimate of the two oxybromides being formed, from which the more volatile monoxybromide can be expelled by a gentle heat. The dioxybromide is not acted upon by water. In the following analyses, the metal was estimated by dissolving the substance in ammonia, evaporating and igniting with access of air; the bromine was determined (collecting the hydrobromic acid formed) by passing hydrogen over the heated substance in a boat; a partial volatilization occurs, but the volatile portion is completely decomposed in the red-hot part of the tube.

	I.	II.	III.	IV.
Weight of substance taken	1·1550	0·9910	1·0077	0·7692
„ tungsten oxide obtained	——	——	0·6265	0·4760
„ silver bromide.....	1·1080	0·9889	——	——
„ silver	0·0131	——	——	——
Percentage of tungsten.....	——	——	49·27	49·07
„ bromine.....	41·65	42·46	——	——

	Calculated.	Found.
Tungsten, W = 184	48·94	49·18
Bromine, Br_2 = 160	42·55	42·05
Oxygen, O_2 = 32	8·51	——
376	100·00	

Iodide of Tungsten.

On passing iodine-vapour, together with carbonic acid, slowly over metallic tungsten heated to redness, a very small quantity of soft scaly crystals, having a greenish metallic lustre, is found to sublime near the heated part of the tube. The same substance is formed (but also only in very small quantities) when iodine-vapour is passed over the heated brown oxide, or a mixture of a metal and an oxide. The product was analyzed by passing air over the heated iodide, when it is readily converted into tungstic trioxide, iodine being volatilized. The sublimed iodine was dissolved in sulphurous acid solution, and the iodide of silver ultimately converted into silver chloride.

Substance taken	0·9142
Tungsten trioxide obtained	0·4953
Silver chloride	0·5850

	Calculated, WI_2.	Found.
Hence tungsten, W = 184	42·01	42·95
„ iodine, I_2 = 254	57·99	56·64
438	100·00	99·59

The iodide is infusible, and cannot be redistilled without decomposition; it is not immediately acted upon by water.

Atomic Weight of Tungsten.

1. *By reduction of Tungsten Trioxide.*—The difficulty of obtaining perfectly pure tungstic acid, and the effect impurity produces on the atomic-weight determinations, has been pointed out by Dumas*.

* Ann. Ch. Pharm. cv. p. 105.

In order to avoid the danger to which all the former determinations are liable, consequent upon the partial reduction to green oxide, which cannot again be oxidized, and the production of which appears to be caused by the presence of traces of alkali, the tungstic acid used was prepared by decomposing pure oxychloride with water. The acid thus obtained has a bright canary-yellow colour, which becomes darker on heating. After igniting a portion of this oxide in a glass tube, in a current of air, it lost its bright yellow colour and assumed a greenish tint, owing to reduction by alkaline impurities which it had taken up from the glass. Hence the acid used for the atomic-weight determinations was ignited on porcelain, and the determination of the loss of weight on reduction and the subsequent gain of weight on oxidation was several times repeated.

The oxide was for this purpose placed in a large porcelain boat (platinum is attacked by tungsten), and heated in a porcelain tube alternately in pure hydrogen and in a current of air. After each reduction the boat was found to be partially coated inside with a thin black film having a metallic appearance, which oxidized completely when heated in the air. A second boat was placed beyond that containing the substance, for the purpose of ascertaining whether any metal was volatilized; but the boat was not found to have become the least discoloured.

The following are the experimental data:—

		grms.
1.	Weighing-tube and boat (after heating for three hours in air) ...	63·0573
2.	„ + tungsten trioxide (after heating for three hours in air).....	70·9413
3.	„ + tungsten (after reduction in hydrogen).....	69·3011
4.	„ + tungsten trioxide (after heating for three hours in air).....	70·9379
5.	„ + tungsten (after reduction in hydrogen)	69·3054
6.	„ + tungsten (after reheating in hydrogen).....	69·3061
7.	„ + tungsten trioxide (after oxidation in air).....	70·9365

Hence we have :—

	grms.
1. Original weight of oxide	7·8840
2. Oxide after first reoxidation.....	7·8806
3. „ second reoxidation.....	7·8792
4. Weight of metal, first reduction	6·2438
5. „ second reduction	6·2481
6. „ third reduction	6·2488

It is evident from these numbers that the second and third weights of oxide and the second and third weights of metal are the only ones which can be relied on as being perfectly pure. Taking the means of these two series, we have 7·8799 grms. of oxide giving 6·24845 grms. metal, or 79·296 per cent. This corresponds to the atomic weight 183·84. In order to have obtained the number 184, 7·8799 grms. of oxide must have yielded 6·24960 grms. of metal, differing by 0·00115 grm. from the experimental observed weight.

2. *From analysis of the Hexachloride.*—Perfectly pure hexachloride was prepared from the pure metal (obtained from the oxychloride), the first product being distilled in a current of chlorine into a clean part of the tube.

No traces of red oxychloride could be detected in this product; and it yielded a perfectly pure canary-yellow trioxide on treatment with water, proving absence of any pentachloride. For the determination of the chlorine, the hexachloride employed was weighed in the piece of drawn-out combustion-tube, in which it was afterwards reduced in hydrogen, the hydrochloric acid being collected and precipitated as silver chloride.

The determination of metal was made in a porcelain boat, in which the weighed quantity of hexachloride was first carefully converted into trioxide by exposure for two days to a moist atmosphere, and afterwards reduced in a current of hydrogen.

The following are the experimental data :—

(A.) *Determination of Chlorine.*

Weight of tungsten hexachloride taken	19·5700	grms.
„ silver chloride obtained	42·3775	„
„ silver on platinum	0·0265	„
Total weight of silver chloride	42·4127	„
Percentage of chlorine	53·605	

(B.) *Determination of Metal.*

Weighing-tube and boat	67·6054	grms.
„ „ and hexachloride	78·0380	„
„ „ and metal	72·4428	„
Weight of chloride	10·4326	„
„ metal	4·8374	„
Percentage of metal	46·368	

Hence the atomic weight of the metal is 184·25; or taking the mean of the two methods, we have 184·04 as the atomic weight of tungsten.

I have, in conclusion, to thank Mr. H. Rocholl for the able assistance which he gave me in carrying out the above experiments.

XI. *Notices of several recently discovered and undescribed British Mosses.* By George E. Hunt, Esq.

Read before the Microscopical and Natural-History Section, Oct. 9th, 1871.

Gymnostomum calcareum, N. & H., var. *brevifolium*, B. & S. (*Gymnostomum viridulum*, Bridel.)

Perennial?, dioicous; stems cæspitose, sparingly branched, very slender, a third of an inch in height, of a reddish-brown colour below, upper part pale green, slightly glaucous; leaves ovate or ovate-lanceolate, with erect bases, thence spreading, papillose, margin crenulated in the

upper part; cells in the upper part of the leaf opaque, quadrangular, in the lower portion elongated, subdiaphanous; nerve thick, papillose, extending almost to the apex. Male flowers gemmiform, on very short axillary branches which usually spring from an innovation; perigonial leaves ovate, suddenly acuminated, nerved to the apex.

I have not seen female flowers or fruit.

Hab. Rocks at Blackhall, near Banchory, where it was discovered by Mr. John Sim.

Entosthodon minimum, Hunt, sp. nov.

Annual, dioicous; stems gregarious, erect, an eighth to a quarter of an inch high; lower leaves obovate, margin reflexed, nerve thin, vanishing below the apex; upper leaves oblong, suberect, subcanaliculate, margin recurved, crenulate in the upper part, nerve rather strong, produced almost to the apex; areolæ large, those of the lower part of the leaf elongate-hexagonal, of the upper part shorter.

Male plants with the flowers terminal, antheridia 6–8, sessile, without paraphyses, perigonial leaves usually like the upper stem-leaves, but occasionally (together with all the stem-leaves) obovate, when they contain clavate, slightly swollen paraphyses, without antheridia.

Female plants with the flowers both terminal and in the axils of the upper stem-leaves; archegonia with a few rather long filiform paraphyses; no distinct perichætial leaves; vaginula short, cylindrical; seta an eighth to a quarter of an inch long, erect; capsule with a distinct neck, smooth, when dry obconical, widest at the mouth; operculum conical acute. Calyptra, when young, brown, very narrow, conical, cleft on one side for a third of its length, cells spirally arranged; peristome half immersed, teeth sixteen, very slender, linear-subulate, transverse articulations distant.

Fruit matures in August.

Hab. Discovered near Glasnevin, Dublin, on the top of a sandstone wall, by Mr. David Orr.

It has no nearly related European allies.

Webera Breidleri, Juratzka (*fide* Fergusson).

Dioicous, growing in extended light-green patches, procumbent in the lower part, which is of a reddish-brown colour; stems about 1½ inch long, leaves ovate, decurrent, erecto-patent, concave, serrated towards the apex, margin recurved; nerve thin, vanishing below the apex; areolæ rather large, upper ones narrow-elongate, acute at both ends, lower ones narrow-elongate, quadrangular. Male flower terminal, discoid; outer perigonial leaves spreading, elliptic-lanceolate, longer than the stem-leaves, saccate at the base, margin strongly recurved, apex cucullate, serrated; inner perigonial leaves obovate, suddenly acuminated, serrated at the diaphanous apex, areolæ large, elongate-quadrangular; antheridia subsessile, with short filiform paraphyses. Perichætial leaves linear lanceolate, recurved at the margin, strongly nerved, nerve vanishing below the apex; seta geniculate near the base, slender; capsule oval, pendulous, glaucous green when young, pale reddish brown when mature.

Fruit matures July to August.

Hab. Abundant on wet *débris* of slaty rocks near springs on the tablelands above the head of Glen Callater, also on Loch-na-gar, and in Canlochan Glen.

Its companions above Glen Callater are *Dicranum Starkii, D. falcatum, Oligotrichum hercynicum,* and *Polytrichum sexangulare.* In the springs themselves abound the following, viz.:—*Philonotis,* several species; *Splachnum vasculosum, Mnium cinclidioides,* and several allied species; *Hypnum exannulatum, H. falcatum, Thuidium decipiens, Webera albicans,* var. *glacialis,* and numerous other interesting plants.

Webera Ludwigii differs in its narrower, hardly concave, patulous leaves, more strongly decurrent, with larger, longer, and more diaphanous areolæ. The whole foliage also is frequently of a fine red colour.

Fruit matures in August.

Hab. Abundant on the fine *débris* of granitic rocks, by streamlets issuing from the perpetual snow-beds near the summits of Ben-mac-Dhui, Ben-na-Boord, and doubtless all the other mountains of like character. On the slaty formations it is rare, and I have only seen it by a steamlet in one small ravine above Glen Callater, where, in the middle of July, the snow was lying abundantly.

Webera Schimperi, Wils. (not of B. & S. Bry. Eur.), has leaves more rigid, erect, narrow-lanceolate, less decurrent, nerve stronger, continued almost to the apex; areolæ a little longer, more obscure.

Fruit matures in July.

Hab. Frequent on *débris* of micaceous rock, on Ben Lawers, and on most of the other Perthshire mountains. It also occurs on *débris* near the summit of Snowdon, but barren.

Philonotis adpressa, Ferg.

Plant widely cæspitose, erect, 2 or 3 inches high, either dull glaucous green or with a fine red tinge; leaves papillose, when moist erect, with one wide plica on each side of the nerve, incurved towards the apex, when dry slightly twisted, widely ovate, from an amplexicaul base, not acuminate, apex either obtuse and cucullate, with a very slight mucro, or in the more slender forms of the plant rather acute; margin denticulate, slightly reflexed; nerve very thick, continuous; cells in the upper part of the leaf small, ovoid, towards the base a little shorter and wider.

I have seen neither flowers nor fruit.

Hab. Glen Prossen, Clova, and various other places in

the Clova district (Rev. J. Fergusson); Glas Mheal, Perthshire, at an elevation of 2500 feet (G. E. Hunt).

In the latter station it was accompanied by *Thuidium decipiens*, De Not.; *Bryum Duvalii*, *Splachnum vasculosum*, and other rare species.

The allies of *Philonotis adpressa* may be distinguished from it as follows:—

Philonotis calcarea has longer, secund, very acute leaves, with areolæ twice or thrice as large, oblong; basal areolæ larger, elongate-hexagonal.

Philonotis fontana has leaves usually spreading, but sometimes secund, longer, suddenly acuminated halfway up, very acute, very distinctly plicate, margin strongly recurved; nerve much thinner; areolæ linear above, small and oblong towards the base of the leaf.

Philonotis seriata, Mitt., has leaves with a distinctly spiral arrangement, from a suberect base, patent towards the apex, ovate, acute, plicate, margin distinctly reflexed; areolæ linear above, small and ovoid towards the base of the leaf; perigonial leaves from an erect dilated base, which is composed of rather large linear cells with a red tinge, upper part of leaf widely spreading, cordate-triangular, obtuse; areolæ elongate-quadrangular, very small and obscure; nerve thick and indistinct, continuous or vanishing below the apex; margin slightly denticulate. This species was first described in Mitten's "Musci Indiæ Orientalis," in the 'Proceedings of the Linnean Society' for 1859. It is frequent in springs at the head of Clova, fruiting freely in favourable seasons.

Thuidium decipiens, De Not. (*Hypnum rigidulum*, Ferg., MSS.)

This species was lately described by the Rev. J. Fergusson in 'Science Gossip,' and noticed in the 'Journal of Botany,' October 1871. It had been collected in 1866

on Ben Lawers by Dr. Stirton, but was for some years confounded with *Hypnum commutatum*, to which species it bears much resemblance. The Rev. J. Fergusson, however, satisfied of its distinctness, distributed it in 1870 as *Hypnum rigidulum*, Ferg., sp. nov.; and a few months since Juratzka identified it with *Thuidium decipiens*, De Notaris, Briologia Italiana, 1869. It occurs in springs, and is found in Britain on Ben Lawers and Glas Mheal, Perthshire—at Auchinblae, Kincardineshire, first observed by Mr. John Sim—and abundantly in various places in Clova and Braemar, first observed by the Rev. J. Fergusson. From every form of *Hypnum commutatum* it is at once separated by its papillose leaves with much dilated auriculate bases, by its larger alar cells, by the ovoid cells of the upper portion of the leaf (those of *H. commutatum* being linear), by its monoicous inflorescence, and by the time of the fruit (which is at maturity in autumn). Fruit has been found only in Italy and in South Prussia.

XII. *Experiments on the Oxidation of Iron.*

By Professor F. Crace-Calvert, Ph.D., F.R.S.

Read January 24th, 1871.

Some two years since Sir Charles Fox inquired of me if I could give him the exact composition of iron-rust, viz. the oxidation found on the surface of metallic iron. I replied that it was admitted by chemists to be the hydrate of the sesquioxide of iron, containing a trace of ammonia; to this he answered that he had read several books on the subject,

in which the statements referring to it differed, and from recent observations he had made he doubted the correctness of the acknowledged composition of rust. He further stated that if he took a bar of rusted wrought iron and put it into violent vibrations by applying at one end the fall of a hammer, scales would be separated which did not appear to him to be the substance I had described.

This conversation induced me to commence a series of experiments which I shall now detail.

I first carefully analyzed some specimens of iron-rust which were procured as far as possible from any source of contamination. Thus, one of these samples was supplied to me by Sir Charles Fox as taken from the outside of the Conway Bridge, the other secured by myself at Llangollen, North Wales.

These specimens gave the following results when submitted to analysis :—

	Conway Bridge.	Llangollen.
Sesquioxide of iron	92·900	93·094
Protoxide of iron.....	6·177	5·810
Carbonate of iron	0·617	0·605
Carbonate of lime	0·295	0·295
Silica.....	0·121	0·196
Ammonia	trace	trace
	100·000	100·000

These results clearly show the correctness of Sir Charles Fox's statements that the composition of the rust of iron is far more complicated than it is stated in our text-books. Therefore the question may be asked, is the oxidation of iron due to the direct action of the atmosphere, or to the decomposition of its aqueous vapour? or does the very small quantity of carbonic acid which it contains determine or intensify the oxidation of metallic iron? To reply to these queries I have made a long series of experiments extending over two years, which I hope will tend to throw some light on this very important question; and although it

appeared to me an easy matter to solve, still I have had many difficulties to overcome, which will be described as they occurred in the course of my investigations.

The first series of experiments consisted in placing crinoline and steel wires, very carefully cleaned, in tubes containing pure dry or moist oxygen; and in another series of tubes, containing the same gas, was added 1 per cent. of carbonic acid or a trace of ammonia.

Before describing the facts observed, I must state that the oxygen was prepared from pure chlorate of potash, mixed with a little binoxide of manganese, and the gas obtained passed through several feet of U-tubes filled with glass moistened with sulphuric acid and caustic potash. The carbonic acid was purified by washing it with water and passing it through U-tubes containing sulphuric acid. The ammonia was purified by first passing it through a saturated solution of ammonia and then over caustic lime. These purified gases were then introduced into tubes having 1 centim. diameter, and 30 centims. in length, which had been previously filled with dry mercury, and containing an iron blade, to the bottom of which had been fixed a small mass of gutta percha, so as to isolate the iron from the mercury, and prevent galvanic currents.

The results of the above experiments were so unsatisfactory that I was led to infer that I had overlooked some source of error; and it was so: small globules of mercury having adhered to the perfectly polished surfaces of the iron, had become centres of galvanic action—determining the oxidation on the blades, and thus rendering the series of experiments discordant.

To overcome this source of error, cleaned blades of steel and iron, having a gutta-percha mass at one end, were introduced into similar tubes to those employed in the previous experiments, and were placed over a mercury-trough; the atmospheric air was displaced by a current of pure oxy-

gen conducted to the top of the experimental tube; and it was then easy to introduce into them traces of moisture, carbonic acid, and ammonia. After a period of four months the blades of iron so exposed gave the following results:—

Dry oxygen	No oxidation.
Damp oxygen	In three experiments, only one blade slightly oxidized.
Dry carbonic acid	No oxidation.
Damp carbonic acid	Slight appearance of a white precipitate on the surface of the iron—found to be carbonate of iron; two only out of six experiments did not give this result.
Dry carbonic acid and oxygen	No oxidation.
Damp oxygen and carbonic acid	Oxidation most rapid, a few hours being sufficient; the blade assumed a dark green colour, which then turned brown-ochre.
Dry oxygen and ammonia	No oxidation.
Damp oxygen and ammonia	No oxidation.

The above results prove that, under the conditions described, pure and dry oxygen does not determine the oxidation of iron—that moist oxygen has only a feeble action—dry or moist pure carbonic acid has no action—but that oxygen containing traces of carbonic acid acts most rapidly on iron, giving rise to protoxide of iron, then to carbonate of the same oxide, and lastly to a mixture of saline oxide and hydrate of sesquioxide.

As these facts tend to show that carbonic acid is the agent which determines the oxidation of iron, I am justified in assuming that it is the presence of carbonic acid in the atmosphere, and not its oxygen or aqueous vapour, which determines the oxidation of iron in common air*. Although this statement may be objected to at first sight, on the ground of the small amount of carbonic-acid gas existing in the atmosphere, still we must bear in mind that a piece of iron, when exposed to its influences, comes

* These results prove the statement of Bonsdorff (see Gmelin, vol. v. p. 185), that carbonic acid has no action on iron, is incorrect.

into contact with large quantities of carbonic acid during twenty-four hours.

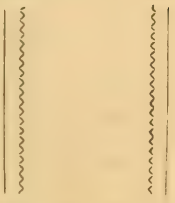
These results appeared so interesting, that I decided to institute several series of experiments.

First Series.—Perfectly cleaned blades of iron placed in bottles filled with ordinary Manchester water were rapidly covered with rust, whilst similar blades, placed in the same water previously deprived of air and carbonic acid by boiling, remained free from rust for a period of four weeks—thus proving that oxygen and carbonic acid are necessary for the production of oxide of iron in presence of water.

Second Series.—Into bottles filled with some pure distilled water (which had been boiled and then allowed to cool in corked flasks) blades of iron were introduced, and a part of the water in the bottles was displaced by common air, pure oxygen, or carbonic-acid gas; but the results were unsatisfactory. I therefore decided to keep the upper part of the blades dry; to effect this, cleaned blades of crinoline-wire were introduced into bottles containing pure oxygen, and then boiled, and cooled distilled water was introduced by means of an aspirator until only half the blades were immersed in the water. By this method I was enabled to ascertain the comparative degree of oxidation of that part of the blades dipping in the water; and the results observed were as follows:—

The section of the blades out of the water remained bright for several days, whilst that in the water became attacked in about six or twelve hours; and the deposit increased so quickly that, after two or three days, the blade was entirely covered with an ochre deposit of the hydrated sesquioxide of iron. This rapid oxidation of iron under these circumstances appeared to me to be entirely due to a galvanic current; for the oxide was deposited in two separate columns, which left the outside edges of the blade perfectly bright, as well as the centre of it, each separate

column apparently representing the poles of a battery. In fact, the production of this precipitate was so rapid that, after a short time, a considerable amount was found as a deposit in the bottle.



To be certain that the oxidation of the immersed portion of the blade was not due to the fixation of oxygen dissolved in the fluid, but to the decomposition of water through a galvanic current, I examined the gaseous mixtures existing in the upper part of the bottle, and found hydrogen in large quantities, thus proving that the water had been decomposed, its oxygen being fixed by the iron, whilst the hydrogen was liberated.

Action of Oxygen and Carbonic Acid on Iron in presence of Water.

To ascertain the influence which carbonic acid exerts on the oxidation of iron, I prepared mixtures of oxygen and carbonic acid in the following proportions:—

25	of oxygen to	75	of carbonic acid.
50	,,	50	,,
75	,,	25	,,
84	,,	16	,,
88	,,	12	,,

Having introduced iron blades and the above gaseous mixtures into bottles, they were inverted over water so that their necks dipped into this fluid. When half the gaseous mixture had been displaced by aspiration, as in the previous experiments, the following results were obtained; viz., whilst that portion of the blade standing out of the water in the previous experiments remained for weeks unoxidized, in this series the same section of the blade assumed rapidly a dark colour, which became afterwards of a dark brown. This change of colour was no doubt due, first, to the for-

mation of carbonate of the protoxide of iron, mixed with carbon existing in the iron; and then the excess of oxygen in the mixture converted the carbonate into magnetic or saline oxide of iron, with a little sesquioxide of iron. Whilst these phenomena were proceeding with that portion of the blade exposed to the gaseous atmosphere, the one dipping into the aqueous solution was observed to loose its brilliant and metallic lustre in a few minutes, presenting a black appearance, which became, at the end of three hours, of a dark greenish hue, due to the formation of similar compounds as those above described.

In all these experiments the water became not only turbid after a short time, but on the surface of the same floated a considerable amount of magnetic oxide and carbonate of the protoxide of iron, and which varied in quantity according to the relative proportions of oxygen and carbonic acid employed. This series of experiments confirms the result of the previous ones, that carbonic acid is a most active agent in determining the oxidation of iron*.

Action of Carbonic Acid on Iron in presence of Water.

Having studied the action of oxygen and of carbonic acid and oxygen on iron in the presence of water, I was desirous of ascertaining what would be the action of carbonic acid and water. I operated in a similar manner as in the previous experiments, and observed that the blades in the gaseous mixtures had assumed a dark grey appearance; in the liquid they were black, the carbon having been rendered perceptible by the iron having been dissolved as carbonate of protoxide of iron, soluble in the excess of the carbonic acid. After a few days, a perfectly white deposit was formed on the edge of the water, which gradually increased and became so abundant that some fell to

* The composition of the various deposits above described was not assumed, but was determined by careful analysis.

the bottom of the vessel. Water had been decomposed and hydrogen liberated.

I again made another experiment, which further illustrates that carbonic acid is the real determining agent of the oxidation of iron. Thus a blade of steel placed in pure water which had been previously deprived of all gaseous mixtures by long boiling, and then allowed to cool, the metal remained bright for several days; and when any signs of oxidation appeared, it was only on a few parts of the blade. The careful examination of this fact led me to infer that the limited action which had taken place might be attributed to traces of impurity in the iron; and this view was supported by the previous observations described, on the influence which minute globules of mercury exert on the oxidation of iron, as well as the known influence which a few thousandths of antimony, platinum, tungsten, sulphur, phosphorus, &c. impart to iron—and, again, the facts I published a few years ago, that by covering the one-hundredth part of a blade of iron with zinc the whole of the blade is prevented from rusting when plunged in soft or sea water, whilst platinum under the same circumstances promotes in a marked degree the oxidation of iron. Therefore, if a minute quantity of a substance modifies the properties of iron so as to facilitate or retard its oxidation, we may, I think, fairly assume that any impurity in the purest steel (watch-springs) may give rise to the few specks of oxide which were observed in the steel placed in boiled distilled water.

Whilst I was engaged in the above experiments, it occurred to me that it might be useful to make a series with the view of throwing light, if possible, on a fact stated by Berzelius, and well known to alkali- and soap-manufacturers, viz. that caustic alkalies have the property, even when dilute, of preventing the oxidation of iron.

Do the alkalies possess the curious property of rendering the iron "passive," or, as Berzelius has supposed, is the iron

in a negative electrical condition? This is a very difficult question to solve. From many observations I have made, I am led to believe that the iron is in a passive state; and this opinion is based not only on the known data, that a blade of iron is not attacked by strong nitric acid, and that we have no electrical current if one fluid and one metal are brought into contact, whilst if two fluids and a metal, or two metals and one fluid, are employed a galvanic current is generated. My experiments show that not only caustic alkalies possess this peculiar property, but also their carbonates and bicarbonates; and I hope that the following experiments will throw some light on this point. A blade of polished iron was dipped into a solution of caustic soda, and at each end of the blade was attached a wire communicating with a galvano-multiplicator, and no galvanic current was generated; and, again, when blades of perfectly polished hoop-iron were placed in bottles half-filled with solutions of caustic alkali, carbonates, and bicarbonates containing from 1 to 5 per cent. of these compounds, and only a part of each blade was immersed in the solution, the other part being exposed to atmospheric air enclosed in the stoppered bottles, the results were, that after three months neither that portion of the blade dipping in the liquid nor that in the atmosphere was in any way rusted in the solutions containing 5 per cent. of alkali; and in the experiments with 1 to $2\frac{1}{2}$ per cent., only the part of the blade out of the water was slightly oxidized.

I was much surprised to find that the carbonates and bicarbonates of the alkalies acted in the same manner as their hydrates in preventing the oxidation of iron, not only on that part of the blade immersed in the solutions but also on that which was exposed to a damp atmosphere of common air. Previously to obtaining these results, I was inclined to believe that caustic alkalies prevented the oxidation of iron by absorbing the carbonic acid of the atmo-

sphere; but, as carbonates and even bicarbonates act in a similar manner, it is evident that this view of the action was erroneous.

The results above stated not coinciding with those obtained by Payen, and given in Gmelin (vol. v. p. 185), I repeated his experiments, and found that the portions of the blades of iron immersed in dilute alkaline solutions as described by him are preserved from oxidation, but that the portion out of the liquor becomes covered with oxide, which increases in inverse ratio to the strength of the solution, and that when the proportion of 1 part of the alkali to 2000 parts of water is reached, the oxidation of the iron proceeds rapidly on that part of the blade just above the surface of the water.

This series of experiments confirms the opinion already expressed that the iron exists in a passive condition, and that this condition only attains sufficient power to resist oxidation by atmospheric oxygen when there is 5 per cent. of either hydrates or carbonates of alkalies in solution.

I have repeated also M. Payen's experiments on the preservative action of weak solutions of carbonate of soda, and find that my results do not correspond with his. He states that 1 part of a saturated solution of carbonate when diluted with 54 parts of water will preserve iron from rust, while 1 with 59 will not. I find that the iron rusts in both cases, thereby proving that stronger solutions are necessary to prevent oxidation.

I have made many experiments with the view of discovering the cause of this interesting preservative action which alkaline solutions exert on iron, but have failed. Knowing the destructive influence which sea-water has on iron, and the serious injury done to iron ships through the action of bilge-water, a series of experiments were made with sea-water to which was added such a quantity of caustic soda or potash or their carbonates that, after the

salts of lime and magnesia were decomposed, there still remained in the solutions 1 to 5 per cent. of alkalies or the alkaline carbonates; and when iron blades were introduced into such liquids, they gave the same results as when iron had been dipped partially into an alkaline solution of Manchester water. I would propose, in conclusion, that a certain quantity of soda-ash should be introduced from time to time into the bilge-water of iron ships, as by so doing a great saving would be effected, as it would prevent the rapid destruction of such ships.

After reading the above paper, Mr. E. Hunt inquired whether I had made experiments to ascertain if the presence of caustic alkalies or their carbonates when mixed with sugar prevented the marked action of that substance on iron. Considering this a most important question to determine, as iron ships are rapidly destroyed when used for conveying sugar from the colonies to this country, I instituted the following series of experiments :—

Blades of iron were partly immersed in a solution containing 10 per cent. of sugar, and in similar solutions to which had been added $2\frac{1}{2}$ to 5 per cent. of caustic and carbonate of soda; after a month the same results were obtained with the saccharine as with the aqueous solutions. But as it would be impracticable to use solutions of caustic or carbonated alkalies in the holds of ships laden with sugar, I thought it therefore advisable to try what might be the influence of zinc when attached to iron in preventing its oxidation, and am glad to say that a piece of zinc covering about the fiftieth part of the blade preserves the iron from rusting in that part of the blade immersed in the sugar-solution. The use, therefore, of plates of zinc fixed on the sides of an iron vessel, or, perhaps better, the employment of galvanized iron plates in the construction of ships, deserves the attention of shipowners.

XIII. *On the Boiling-points of the Normal Paraffins and some of their Derivatives.* By Carl Schorlemmer, F.R.S.

Read February 6th, 1872.

In several papers published in the years 1842, 1844, 1855, &c.*, Professor H. Kopp showed that there exist certain simple relations between the boiling-points and composition of analogous compounds. According to him, the boiling-points of the members of the homologous series of the alcohols, fatty acids, and their compound ethers are raised 19° for each addition of CH_2. Afterwards it was stated that in other series this difference is sometimes smaller and sometimes larger, but that it was always the same in the same series.

In many cases, however, the boiling-points calculated by this rule did not agree at all with those which have been observed. One reason for this discrepancy is that the compounds of which the boiling-points have been compared are not true homologous bodies; *i. e.* they have not an analogous constitution, although they differ in their composition by CH_2 or a multiple thereof. Thus at that time it was believed that ethyl alcohol, propyl alcohol, and butyl and amyl alcohol from fusel-oil, as well as the so-called capryl alcohol from castor-oil, were true homologues; but we know now that this is not the case.

During the last year, however, we have become acquainted with a larger number of true homologous bodies,

* Ann. Chem. Pharm. vol. xli. p. 86, vol. l. p. 182, vol. xcvi. pp. 2, 330, vol. xcviii. p. 267.

as the normal paraffins and the normal alcohols and their derivatives*.

In a paper read before the Royal Society I have already pointed out that the difference between the boiling-points of the lower members of these paraffins is not the same, but that it decreases regularly by 4° until it becomes the well-known difference of 19°, as the following table will show:—

	Boiling-points.		
	Mean found.	Calculated.	Difference.
$C\ H_4$	——	——	——
$C_2\ H_6$	——	——	——
$C_3\ H_8$	——	——	——
$C_4\ H_{10}$	1°	1°	
$C_5\ H_{12}$	38	38	37°
$C_6\ H_{14}$	70	71	33
$C_7\ H_{16}$	99	100	29
$C_8\ H_{18}$	124	125	25
$C_{12}\ H_{26}$	202	201	4×19
$C_{16}\ H_{34}$	278	278	4×19

It appeared to me of interest to compare the boiling-points of other normal compounds, selecting, of course, only such of which the boiling-points have been carefully determined and corrected for pressure and expansion of the mercurial column of the thermometer above the vapour. The result of this investigation is that in most of the other series the difference between the boiling-points also steadily decreases by about 2°; but I am not in a position to state whether this decrease ceases when the difference becomes 19°, as we do not yet know a sufficient number of compounds.

* Compare:—Lieben and Rossi on normal butyl alcohol (Ann. Chem. Pharm. vol. clviii. p. 137), on normal valerianic or pentylic acid (Ann. Chem. Pharm. vol. clix. p. 58), on normal amyl or pentyl alcohol and normal caproic or hexylic acid (Ann. Chem. Pharm. vol. clix. p. 10), Zincke on otyl compounds (Ann. Chem. Pharm. vol. clii. p. 1); Franchimont and Zincke on hexyl compounds (Ann. Chem. Pharm. vol. clxiii. p. 193), and on nonylic acid (Deut. Chem. Ges. Ber. vol. v. p. 19).

Normal Iodides.

	Boiling-points.		
	Observed.	Calculated.	Difference.
	°	°	
Methyl, $C H_3 I$	40	40	°
Ethyl, $C_2 H_5 I$	72	72	32
Propyl, $C_3 H_7 I$	102	102	30
Butyl, $C_4 H_9 I$	129·6	130	28
Pentyl, $C_5 H_{11} I$	155·4	156	26
Hexyl, $C_6 H_{13} I$	179·5	180	24
Heptyl, $C_7 H_{15} I$	——	202	22
Octyl, $C_8 H_{17} I$	221	222	20

Normal Bromides.

	Observed.	Calculated.	Difference.
	°	°	
Ethyl, $C_2 H_5 Br$	39	39	°
Propyl, $C_3 H_7 Br$	71	71	32
Butyl, $C_4 H_9 Br$	100·4	101	30
Pentyl, $C_5 H_{11} Br$	128·7	129	28
Hexyl, $C_6 H_{13} Br$	——	155	26
Heptyl, $C_7 H_{15} Br$	——	179	24
Octyl, $C_8 H_{17} Br$	199	201	22

Normal Chlorides.

	Observed.	Calculated.	Difference.
	°	°	
Ethyl, $C_2 H_5 Cl$	12·5	13	°
Propyl, $C_3 H_7 Cl$	46·4	46	33
Butyl, $C_4 H_9 Cl$	77·6	77	31
Pentyl, $C_5 H_{11} Cl$	106·6	106	29
Hexyl, $C_6 H_{13} Cl$	——	133	27
Heptyl, $C_7 H_{15} Cl$	——	158	25
Octyl, $C_8 H_{17} Cl$	180	181	23

Normal Acetates.

	Observed.	Calculated.	Difference.
	°	°	
Ethyl, $C_4 H_8 O_2$	74	74	°
Propyl, $C_5 H_{10} O_2$	102	101	27
Butyl, $C_6 H_{12} O_2$	125·1	126	25
Pentyl, $C_7 H_{14} O_2$	148·4	149	23
Hexyl, $C_8 H_{16} O_2$	168·7	170	21
Heptyl, $C_9 H_{18} O_2$	——	189	19
Octyl, $C_{10} H_{20} O_2$	207	208	19

Whilst in these series the difference between the boiling-points steadily diminishes, in the series of the normal alcohols the difference appears to remain the same, being about 19°.

Normal Alcohols.

	Observed.	Calculated.
	°	°
Ethyl, C_2H_6O	78·4	78·4
Propyl, C_3H_8O	97	97
Butyl, $C_4H_{10}O$	116	116
Pentyl, $C_5H_{12}O$	137	135
Hexyl, $C_6H_{14}O$	156·6	154
Heptyl, $C_7H_{16}O$	——	173
Octyl, $C_8H_{18}O$	192	192

In the series of the normal fatty acids, the difference between the boiling-points of the lower members is also constant, being 22°, but afterwards it becomes less.

Normal Fatty Acids.

	Observed.	Calculated.	Difference.
	°	°	°
Acetic, $C_2H_4O_2$	118	118	
Propionic, $C_3H_6O_2$	140·6	140	22
Butyric, $C_4H_8O_2$	163·2	162	22
Pentylic, $C_5H_{10}O_2$	184·5	184	22
Hexylic, $C_6H_{12}O_2$	204·5	206	22
Heptylic, $C_7H_{14}O_2$	220		
Octylic, $C_8H_{16}O_2$	233		
Nonylic, $C_9H_{18}O_2$	254		

XIV. *On the Inverse, or Inductive, Logical Problem.*

By W. Stanley Jevons, M.A., F.R.S., Professor of Logic in The Owens College, Manchester.

Read December 26th, 1871.

Logical deduction consists in ascertaining from a law or laws the combinations of qualities which may exist under those conditions. The natural law that all metals are conductors of electricity really means that in nature we find three classes of objects, namely:—(1) metals, conductors; (2) not metals, conductors; (3) not metals, not conductors. It comes to the same thing if we say that it excludes the existence of the class metals not conductors. But every scientific process has its inverse process. As addition is undone by subtraction, multiplication by division, involution by evolution, differentiation by integration, so logical induction is the inverse process of deduction. Given certain classes of objects, we endeavour by induction to pass back to the laws embodied in those classes; given combinations, we have to learn the laws obeyed by them. There does not exist, indeed, any distinct method of induction, except such as consists in inverting the processes of deduction, by noting and remembering the laws from which certain effects necessarily follow. The difficulties of induction are thus exactly analogous to those of integration, which can only be performed by trial, assisted by a full knowledge of the effects of differentiation and a certain happy knack of arranging the formulæ so as to bring them into connexion with some previous result of the direct process.

In two essays*, and in my paper "On the Mechanical Performance of Logical Inference"†, I have attempted to represent, with the utmost generality and simplicity, the processes of formal logic by which, from any proposition or series of propositions, we arrive at the combinations of terms possible under the condition of their truth. The inverse problem yet remains, I believe, to be considered. Given certain combinations, what are the propositions stating their conditions? I proceed to explain how this problem can be resolved in the case of two or three terms.

According to the laws of thought, two terms, say A and B, can be present or absent in four combinations, thus—

$$AB, \quad Ab, \quad aB, \quad ab.$$

A small italic letter indicates the absence or negation of the corresponding large one. The above combinations are unconditioned, except by the primary conditions of thought itself; but if we remove any one of the combinations, say Ab, the meaning will be that A *which is not* B cannot exist. Thus, the three combinations AB, aB, ab being given, we pass back to the law all A's are B's, or, if A mean metal and B conductor, to a law such as "all metals are conductors."

We arrive at the utmost number of cases which can occur by omitting any one or more of the four combinations. The number of possible cases is therefore $2 \times 2 \times 2 \times 2$, or 16; and they are all shown in the following table, in which the sign o indicates the non-existence of the combination given at the left hand, and the mark 1 its presence.

* Pure Logic: London, 1864 (Stanford). The Substitution of Similars: London, 1869 (Macmillan).

† Philosophical Transactions, 1870, vol. clx. p. 497.

	1.	2.	3.	4.	5.	6.	7. *	8. *	9.	10. *	11.	12. *	13.	14. *	15. *	16. *
AB	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1	1
A*b*	0	0	0	0	1	1	1	1	0	0	0	0	1	1	1	1
*a*B	0	0	1	1	0	0	1	1	0	0	1	1	0	0	1	1
ab	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1

Thus column 16 represents the case where all combinations are present, and the only conditions are the laws of thought. The example of metals and conductors of electricity would be represented in the 12th column; and every other mode in which two things or qualities might present themselves together or apart, is shown in one column or another. But more than half the cases may at once be rejected because they involve the entire absence of a term or its negative. Thus, in the 1st column, no combinations at all are represented as present. In column 2 there is only the negative combination *ab*. Now it is a logical principle that when any term or its negative entirely fails to appear, there must be some contradiction between the conditions of combination. Thus the two conditions A is B and A is not B, would result in altogether destroying the combinations containing A. We may therefore restrict our attention to those cases where at least two combinations are present, and they contain among them each of the letters A, B, *a*, *b*; these cases are represented in the columns marked with the sign *. Among these seven cases we find

Four cases containing three combinations,
Two cases containing two combinations,
One case containing four combinations.

It has already been pointed out that a proposition of the form A is B, or, as it is more exactly represented in a symbolic notation explained in the essays referred to, A=AB, destroys one combination A*b*, and thus accounts for the 12th case. Let us consider in how many ways we

can vary this form of proposition either by interchanging A and B or substituting for one or both of them its negative. We should arrive altogether at eight different expressions, which are thus stated:—

12.	8.	15.	14.
$A=AB,$	$A=Ab,$	$a=aB,$	$a=ab,$
$b=ab,$	$B=aB,$	$b=Ab,$	$B=AB.$

If we test the effect of each of these conditions by ascertaining the combinations which it negatives, it will be found that each proposition in the first line gives the same result as the one immediately below it. Each pair consists then of logical equivalents. And in fact the lower line contains what are called by logicians the contrapositives of those in the higher line. Thus the first pair mean

All A's are B's,
All not B's are not A's,

which have exactly the same logical force. The last pair mean

All not A's are not B's,
All B's are A's,

which are again exactly equivalent to each other. Although then there may be eight propositions of the form $A=AB$, only four of these have independent logical meanings. On trial we find that these four propositions give the combinations respectively shown in the 12th, 8th, 15th, and 14th columns, which cases are thus referred to their proper law.

If we now join these latter four propositions two and two, they will generally be found to contradict each other; thus all A's are B's contradicts all A's are not B's. There are only two pairs which give consistent results, namely:—

$$\left.\begin{matrix} A=AB \\ a=ab \end{matrix}\right\}, \quad \text{and} \quad \left.\begin{matrix} A=Ab \\ a=aB \end{matrix}\right\}.$$

The first of these pairs gives the combinations in the 10th column; but it may readily be shown that the two propositions all A's are B's, and all not A's are not B's, are but equivalent to the single proposition A=B, or all A's are all B's. In the same way the second pair of propositions gives the combinations of the 7th column, and are equivalent to the single proposition A=b, or all A's are all not B's. There remains but a single case, that in the 16th column; but as all the combinations are present, there can be no condition except the laws of thought.

We have now effected a complete solution of the inverse logical problem of two terms; we have found that two terms can manifest themselves only in seven series of combinations, and the corresponding laws are as below:—

Series of combinations.	Laws.	Equivalent laws.
12th	A=AB	b =ab
8th	A=Ab	B=aB
15th	a =aB	b =Ab
14th	a =ab	B=AB
10th	A=B	a =b { A=AB ; a =ab
7th	A=b	a =B { A=Ab ; a =aB
16th	No law	

We also learn from the above investigation that there is no possible logical relation between two terms which may not be expressed in a proposition either of the general type A=AB or of A=B. For the logical relation must manifest itself in some series of combinations of natural qualities; but every series of combinations which is possible, according to the very laws of thought, has been included in our investigation. Thus every such logical relation must either be expressed in one of the six laws, or must be equivalent to one of them. The general result

of the problem then is that two terms admit only of six distinct logical relations, which again have only two essentially different typical forms, namely, A = AB and A = B. These laws express respectively partial and complete coincidence; the first is illustrated by the relation between *metals* and *elements*, the metals coinciding with a part of the elements; the latter, by the complete coincidence between substance possessing inertia, and substance possessing gravity, or between crystals of the cubical system and crystals not capable of doubly refracting light.

The Inverse Logical Problem involving Three Terms.

No sooner do we introduce into the problem a third term C, than the investigation assumes a far more complex character. There are now three terms, A, B, C, and their negatives, *a*, *b*, *c*, which may be combined, according to the Laws of Thought, in eight different combinations, namely,

ABC,	*a*BC,
AB*c*,	*a*B*c*,
A*b*C,	*ab*C,
A*bc*,	*abc*.

The effect of any logical conditions is to destroy one or more of these combinations. Now we may make selections from eight things in 2^8, or 256 ways; so that we have no less than 256 different cases to treat, and the complete solution is at least fifty times as troublesome as with two terms. Many series of combinations indeed are contradictory, as in the simpler problem, and may be passed over. The test of consistency is that each of the letters A, B, C, *a*, *b*, *c* shall appear somewhere in the series of combinations; but I have not been able to discover any mode of calculating the number of cases in which inconsistency would happen. The logical complexity of the problem is so great that the

ordinary modes of calculating numbers of combinations in mathematical science fail to give any aid, and exhaustive examination of the combinations in detail is the only method applicable.

My mode of solving the problem was as follows:— Having written out the whole of the 256 series of combinations, I examined them separately, and struck out such as did not fulfil the test of consistency. I then chose some common form of proposition involving two or three terms, and varied it in every possible manner, both by the circular interchange of letters (A, B, C into B, C, A and then into C, A, B) and by the substitution for any one or more of the terms of the corresponding negative terms. For instance, the proposition AB = ABC can be first varied, by circular interchange, so as to give BC = BCA and then CA = CAB. Each of these three can then be thrown into eight varieties by negative change. Thus, AB = ABC gives aB = aBC, Ab = AbC, AB = ABc, ab = abC, and so on. Thus there may possibly exist no less than twenty-four varieties of the law having the general form AB = ABC, meaning that whatever has the properties of A and B has those also of C. It by no means follows that some of the varieties may not be equivalent to others; and trial shows, in fact, that AB = ABC is exactly the same in meaning as Ac = Acb or Bc = Bca. Thus the law in question has but eight varieties of distinct logical meaning. I now ascertain, by actual deductive reasoning, which of the 256 series of combinations result from each of these distinct laws, and mark them off as soon as found. I now proceed to some other form of law, for instance A = ABC, meaning that whatever has the qualities of A has those also of B and C. I find that it admits of twenty-four variations, all of which are found to be logically distinct; the combinations being worked out, I am able to mark off twenty-four more of the list of 256 series. I proceed in this way to work out the

results of every form of law which I can find or invent. If in the course of this work I obtain any series of combinations which had been previously marked off, I learn at once that the law is logically equivalent to some law previously treated. It may be safely inferred that every variety of the apparently new law will coincide in meaning with some variety of the former expression of the same law; I have sufficiently verified this assumption in some cases, and have never found it lead to error. Thus, just as AB=ABC is equivalent to Ac=Abc, so we find that ab=abC is equivalent to ac=acB.

Among the laws treated were the two A=AB and A=B, which involve only two terms, because it may of course happen that among three things two only are in special logical relation, and the third independent; and the series of combinations representing such cases of relation are sure to occur in the complete enumeration. All single propositions which I could invent having been treated, pairs of propositions were next investigated. Thus we have the relations "all A's are B's, and all B's are C's," of which the old logical syllogism is the development. We may also have "all A's are all B's, and all B's are C's," or even "all A's are all B's, and all B's are all C's." All such premises admit of variations, greater or less in number, the logical distinctness of which can only be determined by trial in detail. Disjunctive propositions, either singly or in pairs, were also treated, but were often found to be equivalent to other propositions of a simpler form; thus A=B ·|· C (the sign ·|· standing for the disjunctive conjunction *or* in an unexclusive sense) is exactly the same in meaning as a=bc.

This mode of exhaustive trial bears some analogy to that ancient mathematical process called the sieve of Eratosthenes. Having taken a long series of the natural numbers, Eratosthenes is said to have calculated out in succes-

sion all the multiples of every number, and to have marked them off, so that at last the prime numbers alone remained, and the factors of every number were exhaustively discovered. My problem of 256 series of combinations is the logical analogue, the chief points of difference being that there is a limit to the number of cases, and that prime numbers have no analogue in logic, since every series of combinations corresponds to some law or group of conditions. But the analogy is perfect in the point that they are both inverse processes. There is no mode of ascertaining that a number is prime but by showing that it is not the product of any assignable factors. So there is no mode of ascertaining what laws are embodied in any series of combinations but trying exhaustively the laws which would give them. Just as the results of Eratosthenes's method have been worked out to a great extent and registered in tables for the convenience of other mathematicians, I have endeavoured to work out the inverse logical problem to the utmost extent which is at present practicable or useful.

I have thus found that there are altogether fifteen conditions or series of conditions which may govern the combinations of three terms, forming the premises of fifteen essentially different kinds of arguments. The following table contains a statement of these conditions, together with the number of combinations which are contradicted or destroyed by each, and the number of logically distinct variations of which the law is capable. There might be also added, as a sixteenth case, that case in which no special logical condition exists, so that all the eight combinations remain.

Reference Number.	Propositions expressing the general type of the logical conditions.	Number of distinct logical variations.	Number of combinations contradicted by each.
I.	A=B	6	4
II.	A=AB	12	2
III.	A=B, B=C	4	6
IV.	A=B, B=BC	24	5
V.	A=AB, B=BC	24	4
VI.	A=BC	24	4
VII.	A=ABC	24	3
VIII.	AB=ABC	8	1
IX.	A=AB, aB=aBc	24	3
X.	A=ABC, ab=abC	8	4
XI.	AB=ABC, ab=abc	4	2
XII.	A=ABC $\cdot\vert\cdot$ Abc	12	2
XIII.	A=BC $\cdot\vert\cdot$ Abc	8	3
XIV.	A=BC $\cdot\vert\cdot$ bc	2	4
XV.	A=ABC, a=Bc $\cdot\vert\cdot$ bC	8	5
		192	

There are 63 series of combinations derived from self-contradictory premises, which, with the above 192 series and the one case where there are no conditions or laws at all, make up the whole conceivable number of 256 series.

We learn from this table, for instance, that two propositions of the form A=AB, B=BC, which are such as constitute the premises of the old syllogism Barbara, negative or render impossible four of the eight combinations in which three terms may be united, and that these propositions are capable of taking twenty-four variations by transpositions of the terms or the introduction of negatives. This table then presents the results of a complete analysis of all the possible logical relations arising in the case of three terms, and the old syllogism forms but one out of fifteen typical forms. Generally speaking, every form can be converted into apparently different propositions; thus the fourth type A=B, B=BC may appear in the form A=ABC, a=ab, or again in the form of three propositions A=AB, B=BC, aB=aBc; but all these sets of premises yield identically the same series of combinations, and are, therefore, of exactly equivalent logical meaning. The fifth

type, or Barbara, can also be thrown into the equivalent form $A=ABC, aB=aBC$. In other cases I have obtained the very same logical conditions in four modes of statement. As regards mere appearance and mode of statement, the number of possible premises would be almost unlimited.

The most remarkable of all the types of logical conditions is the fourteenth, namely $A=BC \cdot|\cdot bc$. It is that which expresses the division of a genus into two doubly marked species, and might be illustrated by the example—Component of the physical Universe = Matter, gravitating, ·|· Not matter (ether), not gravitating.

It is capable of only two distinct logical variations, namely $A=BC \cdot|\cdot bc$ and $A=Bc \cdot|\cdot bC$. By transposition, or negative change of the letters, we can indeed obtain six different expressions of each of these propositions; but when their meanings are analyzed by working out the combinations, they are found to be logically equivalent to one or the other of the above two. Thus the proposition $A=BC \cdot|\cdot bc$ can be written in any of the following five other modes:—

$$a=bC \cdot|\cdot Bc, \qquad B=CA \cdot|\cdot ca, \qquad b=cA \cdot|\cdot Ca,$$
$$C=AB \cdot|\cdot ab, \qquad c=aB \cdot|\cdot Ab.$$

I do not think it needful at present to publish the complete table of 193 series of combinations and the premises corresponding to each. Such a table enables us by mere inspection to learn the laws obeyed by any set of combinations of three things, and is to logic what a table of factors and prime numbers is to the theory of numbers, or a table of integrals to the higher mathematics. The table already given above (p. 128) would enable a person with but little labour to discover the law of any combinations. If there be seven combinations (one contradicted) the law must be of the eighth type, and the proper variety will be apparent; if there be six combinations (two contradicted), either the

second, eleventh, or twelfth type applies, and a certain number of trials will disclose the proper type and variety; if there be but two combinations, the law must be of the third type; and so on.

The above investigations are complete as regards the possible logical relations of two or three terms. But when we attempt to apply the same kind of method to the relations of four or more terms, the labour becomes impossibly great. Four terms give sixteen combinations compatible with the laws of thought, and the number of possible selections of combinations is no less than 2^{16}, or 65,536. The following table shows the extraordinary manner in which the number of possible logical relations increases with the number of terms involved.

Number of terms.	Number of possible combinations.	Number of possible selections of combinations corresponding to consistent or inconsistent logical relations.
2	4	16
3	8	256
4	16	65,536
5	32	4,294,967,296
6	64	18,446,744,073,709,551,616

Some years of continuous labour would be required to ascertain the precise number of types of laws which may govern the combinations of only four things; and only a small part of such laws would be exemplified or capable of practical application in science. The purely logical inverse problem whereby we pass from combinations to their laws is solved in the preceding pages as far as it is likely to be for a long time to come; and it is almost impossible that it should ever be carried more than a single step further.

XV. *Corrections of the Nomenclature of the Objects figured in a Memoir "On some of the minute Objects found in the Mud of the Levant" &c.**. By Professor W. C. Williamson, F.R.S.

Read April 30th, 1872.

In 1845 I laid before the Manchester Philosophical Society a memoir "On some of the Microscopic Objects found in the Mud of the Levant and other Deposits; with Remarks on the Mode of Formation of Calcareous and Infusorial Siliceous Rocks." This memoir was written at the period when the microscope was opening out a new world alike to the zoologist, the botanist, and the geologist. The study of the Foraminifera was beginning to assume its modern scientific form; and the objects now so familiar to us as Diatomaceæ were being discovered at innumerable localities; so that we were entering upon that harvest of beautiful organisms which we have since reaped in so abundant a measure.

But, as was inevitable in the infancy of these studies, our views on many points connected with them were vague and unformed. The memoir in question constitutes one of the earliest of the attempts made to give scientific shape to a number of important problems, especially dealing with the nature of the Foraminifera, demonstrating the organic nature of some sea-beds and the almost invariable organic origin of Limestones, insisting upon the Diatomaceous character of those innumerable microscopic disks of which *Coscinodiscus, Campylodiscus,* and *Actinocyclus* are amongst

* Published in vol. viii. of the 'Memoirs of the Literary and Philosophical Society of Manchester' (1847).

the earliest known of the many types with which the labours of Ehrenberg made the scientific world familiar.

At the above period the nomenclature of these various objects was in a most unsettled state; consequently the specific references contained in the above memoir are very defective. Some of my fellow-labourers in this field are kind enough to say that the memoir still retains sufficient scientific value to make a correction of the nomenclature a desirable thing. My valued friend Professor Rupert Jones has not only urged me to undertake this task, but, by the assistance he has kindly given me in the case of the recent and fossil Foraminifera, he has made the duty a light one. The following columns therefore give, on the one hand, references to the figures of the various objects represented in the plates to the memoir in question, and, on the other, the names by which these objects are known at the present day.

Diatomaceæ.

References to the Memoir.	Modern Nomenclature.
Plate I.	
fig. 1. *Denticella tridentata*, Ehr. (p. 16) ...	*Biddulphia Tuomeyi.*

It is my belief that *Biddulphia regina* (Smith), *tridentata* (Ehrenberg), and *Tuomeyi* (Bailey) are but varieties of the same species. They chiefly vary in the length of their terminal processes.

fig. 2. *Biddulphia pulchella*, Gray (p. 16)...	*Biddulphia pulchella.*
3. *Amphitetras antediluviana*, Ehr. (p. 16)	*Amphitetras antediluviana.*
4, 6, 8. *Grammatophora africana*, Ehr. (p. 17)	*Grammatophora serpentina.*
5. —— ——, var. (p. 18)	

This not uncommon marine species varies exceedingly in the form of its frustules and in the number of the undulations of its characteristic vertical septa—*G. angulosa*, Ehr., *africana*, Ehr., *anguina*, Kützing, *mediterranea*, Ehr., and *islandica*, Ehr., being but varieties of the same

species. We have here a very characteristic example of that undue multiplication of species to which Ehrenberg has been so prone.

References to the Memoir.	Modern Nomenclature.
Plate I.	
f. 7,9. *Grammatophora tæniæformis* (p. 17) ...	*Grammatophora marina*, Lyng.

According to Kützing, *Diatoma tæniæforme, latruncularium* (Agardh), *brachygonium* (Carm.), *Bacillaria Cleopatræ* (Ehr.), *B. adriatica, B. Meneghina* (Lobarewsky), and *Grammatophora oceanica* are all varieties of this species; and I doubt the distinctness of *macilenta* (Smith) and some others.

fig. 10. ?	
11. *Striatella arcuata*, var. (p. 18)	*Rhabdonema arcuata*, var.
12. *Fragillaria* (p. 18) ...	Probably a thick variety of *Fragillaria capuana* (Desm.).
13. *Gomphonema geminatum*? (p. 18)	Both doubtful.
14. —— (p. 18)	
15. *Navicula* (p. 20)	*Navicula didyma* (Kütz.).
16. —— (p. 20)	All uncertain species.
17. —— (p. 20)	
18. —— (p. 20)	
19. —— (p. 20)	
20. —— *sigma*	*Pleurosigma* (species uncertain).
21. —— (p. 20)	
Plate II.	
f. 22. *Surirella* (p. 20)	
Plate I.	
f. 23. *Cocconeis* (p. 20)	*Navicula.*
24. —— (p. 20)	*Navicula.*
Plate II.	
f. 25. *Cocconema* (p. 21)	*Cocconema*, or *Cymbidum.*

It is impossible to distinguish isolated frustules of these two genera, the chief distinction being that the one is supported on a peduncle, whilst the other is free.

f. 26. *Campylodiscus* (p. 51).....	*Campylodiscus* (undescribed).

Foraminifera (*Recent*).

f. 27. *Rosalina globularis*, D'Orb., or *Rotalia stigma*, Ehr. (pp. 38–43)	Young individual of *Planorbulina mediterranensis.*

References to the Memoir.	Modern Nomenclature.
Plate II.	
f. 28. *Rosalina globularis*, D'Orb., or *Rotalia stigma*, Ehr., older (pp. 38–43)	More matured *Planorbulina mediterranensis.*
29. Decalcified animal of the same.	

In reasoning upon this specimen in the memoir (pages 38–39), I erroneously concluded that the form which it assumed afforded proof that its external surface consisted of a membrane more dense than the rest of the structure. I first became convinced that my reasoning was inaccurate from observing that some collodion dried up in a bottle in precisely the same way as the gelatinous sarcode of the Foraminifer dried up within the interior of each segment of the shell; *i. e.* it first shrank considerably in size, its external surface assuming a hard membranous aspect, after which the contraction ceased in the centripetal direction. The soft contained collodion now contracted from within outwards upon the hardened exterior, which ultimately assumed the form and aspect of a dense membrane enclosing a hollow cavity. This is precisely what we see in these dried-up Foraminifera.

fig. 30. *Polystomella* (p. 44)	*Polystomella striatopunctata* (Fichtel & Moll).
31. *Peneroplis* (p. 44)	*Peneroplis pertusus*, Forskål.

This is the species so widely known as *P. planatus*, the specific name given to it by Fichtel and Moll.

f. 32. *Peneroplis*, young shell (p. 45)	*Peneroplis pertusus.*
33. *Textilaria* (p. 47)	*Bolivina punctata*, D'Orb.
34. *Foraminifer* (p. 45)	*Spirillina perforata* (Williamson).
Plate III.	
f. 35. *Truncatulina tuberculata* (p. 46) ...	*Truncatulina lobatula.*
36. *Spiroculina* (p. 46)	Being very young examples, it is almost impossible to determine the species of these three *Miliolæ.*
37. —— (p. 46).....	
38. *Biloculina* (p. 46)	
39. *Lagena globosa*? (p. 47)	*Lagena globosa* (Montagu).

Sponge-spicula.

References to the Memoir.	Modern Nomenclature.
Plate III.	
f. 40. Spiculum of *Tetheia* (p. 26)	Spiculum of a sponge.
41. Ditto (p. 26)	Spiculum of *Tetheia*.
42. Globular cortical spicule of *Geodia* (p. 27).....	Cortical spicule of *Geodia*.
43. Spicule of ? (p. 27).....	Sponge spiculum.
44. ?	Ditto.
45. Calcareous spicule of ? (p. 26)	Ditto, calcareous.
46. Siliceous spicule of *Halichondria* (p. 25)	Siliceous acicular spiculum of sponge.
47. Ditto (p. 25)	Siliceous capitate ditto.
48. Muricated spicule of sponge (p. 27) .	Siliceous muricate ditto.
49. Calcareous spicule of one of the *Eolidæ*? (p. 27)	Calcareous spiculum of *Gorgonia*.

Miscellaneous.

fig. 50. Rudimentary plate of an Echinoderm (p. 53)	Rudimentary plate of an Echinoderm.
51. Ditto (p. 53)	Ditto.
52. Ditto (p. 53)	Ditto.
53. Cribriform plate (fragment) of a matured Echinoderm (p. 53) ...	Fragment of test of a matured Echinoderm.
54. Lamina of *Pinna*-shell (p. 52) ...	Lamina of *Pinna*-shell, viewed superficially.
55. Portion of a detached prism of the same (p. 52)	Portion of a detached prism of the same.

Cretaceous Foraminifera.

Plate IV.	
f. 56. *Rotalia* (p. 75).....	*Planorbulina Lorneiana* (D'Orb.).
57. Ditto (p. 75)	Ditto.
58. *Robulina*, or *Cristellaria* (p. 75) ...	*Cristellaria rotulata* (Lam.).
59. ? (p. 75) ...	*Globigerina bulloides*?

Professor Rupert Jones informs me that he has seen a state of *Globigerina bulloides* from the Atlantic ooze like the above figure. At the same time I am doubtful respecting the identity of the fossil with the well-known living form.

	References to the Memoir.	Modern Nomenclature.
Plate IV.		
f. 60.	*Rotalia globulosa*, Ehr. (p. 76)	*Globigerina cretacea*, D'Orb.
61.	*Textilaria globulosa*, Ehr. (p. 76) ...	*Textilaria gibbosa* (D'Orb.).
62.	*Textilaria* (p. 76)	—— *prælonga* (Reuss).
63.	*Verneuilina* (p. 77).....	*Verneuilina triquetra* (Münster).
64.	*Textilaria*, genus near to (p. 76) ...	*Textilaria trochus.*
65.	Ditto (p. 76)	Ditto.
66.	*Marginulina* (p. 77)	*Vaginulina costulata* (Reuss).
67.	*Cristellaria* (p. 77)	*Cristellaria* (*Planularia*) *complanata* (Reuss).
68.	Ditto (p. 77)	Ditto, ditto, *cymba* (D'Orb.).
69.	*Dentalina* (p. 78)	Small bent *Nodosaria raphanus* (Linn.), or *N. obscura* (Reuss).
70.	Ditto (p. 78)	*Dentalina communis*, D'Orb.
71.	*Lagena* (p. 78).....	Coccolith.

When investigating the cretaceous deposits I found vast numbers of these small bodies, which, when viewed by transmitted light, appeared to have a single central cavity like that of the recent *Lagenæ*. There is no doubt, however, that these are the Coccoliths to which so much attention has been given during the last three or four years. The above figure is a bad representation of the external aspect of one of these small objects.

fig. 72.	*Dentalina* (p. 78)	*Dentalina communis* (D'Orb.).
73.	*Dentalina aculeata* (D'Orb.) (p. 78).	—— *aculeata* (D'Orb.).
74.	Ditto.....	Ditto.

Cretaceous Entomostraca.

f. 75.	*Cytherina echinulata*, Williamson (p. 78)	*Cythere* (*Cythereis*) *ornatissima* (Reuss)*.
76.	(p. 78)	
77.	—— *concentrica*, W. (p. 79)	*Cythere concentrica* (Roemer).
78.	—— *umbonata*, W. (p. 79)	*Cytheroptera umbonata* (W.).
79.	—— *serrata*, W. (p. 79).....	*Cytherella Williamsoniana* (?) (Jones).
80.	—— *lævis*, W. (p. 79)	—— *Muensteri* (Roemer).

* See Geological Magazine, no. 68, p. 75.

Sponge-spicula.

References to the Memoir.	Modern Nomenclature.
Plate IV.	
f. 81. Sponge-spiculum? (p. 80)	Sponge-spiculum.
82. Ditto, calcareous (p. 80)	Ditto, calcareous.
83. Ditto, siliceous (p. 80)	Ditto, siliceous.

Shell-structures.

f. 84. Laminæ of shell-structure (*Inoceramus*?) (p. 80)	Lamina of *Inoceramus* (shell).
85. Detached prisms of ditto (p. 80) ...	Prisms of ditto.
86. Ditto (p. 80)	Ditto.

XVI. *On Changes in the Distribution of Barometric Pressure, Temperature, and Rainfall under different Winds during a Solar-spot Period.* By Joseph Baxendell, F.R.A.S.

Read before the Physical and Mathematical Section, November 7th, 1871.

In my paper "On Periodic Changes in the Magnetic Condition of the Earth, and in the Distribution of Temperature on its Surface," read March 8, 1864, I endeavoured to account for some of the phenomena therein described by assuming that variations in the magnetic condition of the earth produce corresponding variations in the direction and velocity of the great currents of the atmosphere; and some time afterwards, in considering this hypothesis more carefully, it appeared to me that if, as is generally supposed, magnetic changes are intimately connected with the disturbances which take place in the solar photosphere, their influence upon the atmosphere ought to be indicated by variations in the distribution of baro-

metric pressure, temperature, and rainfall under different winds corresponding to the variations of solar-spot frequency. Fortunately the means of at once testing the soundness of this view were at hand in the valuable tables numbered XVI. and XVIII. in the volumes of the 'Radcliffe Observations,' which show for each year the relations between barometric pressure, temperature, and rainfall under different winds at Oxford. I therefore extracted from these tables, and arranged in order, the mean annual barometric pressures, mean temperatures, and amounts of rainfall under different winds for the ten years 1858–67; and on carefully examining the table thus formed I found that changes had taken place in the three elements which corresponded very closely in the times of their maxima and minima with those of solar-spot frequency.

The mean length of a solar-spot period is about 11 years and 5 weeks; and as the volume of 'Radcliffe Observations' for 1868 has been published since I formed the ten-years' table, I have included the mean results for that year in the following table, which thus represents the changes which took place through a complete solar-spot period.

According to the observations of Schwabe the numbers of groups of solar spots which occurred in the years 1858–68 were as follows:—

Year.	Number of groups.	Year.	Number of groups.
1858	188	1864	130
1859	205	1865	93
1860	211	1866	45
1861	204	1867	25
1862	160	1868	101
1863	124		

The mean number is 135; and therefore it appears that during the five years 1858–62 the frequency of solar spots was above the average, and during the six years 1863–68 it was below. In order, then, to ascertain the effects of

Barometric Pressure, Temperature, and Rainfall under different Winds, at Oxford, during the Years 1858–68.

Year.	N.			N.E.			E.			S.E.			S.			S.W.			W.			N.W.		
	Mean barometric pressure.	Mean temperature.	Total rainfall.	Mean barometric pressure.	Mean temperature.	Total rainfall.	Mean barometric pressure.	Mean temperature	Total rainfall.	Mean barometric pressure.	Mean temperature.	Total rainfall.	Mean barometric pressure.	Mean temperature.	Total rainfall.	Mean barometric pressure.	Mean temperature.	Total rainfall.	Mean barometric pressure.	Mean temperature.	Total rainfall.	Mean barometric pressure.	Mean temperature.	Total rainfall.
	in.	°	in.	in.	°	in.	in.	°	in.	in.	°	in.	in.	°	in.	in.	°	in.	in.	°	in.	in.	°	in.
	29+			29+			29+			29+			29+			29+			29+			29+		
1858.	·955	47·9	1·80	·923	46·5	3·24	·866	47·3	4·65	·756	50·2	2·02	·692	52·3	4·30	·629	51·6	7·31	·670	49·1	4·22	·850	46·7	1·25
1859.	·861	47·1	5·29	·835	48·1	5·89	·832	49·7	3·14	·773	51·1	3·79	·612	52·6	9·63	·622	52·6	12·30	·723	48·9	3·28	·860	46·9	1·88
1860.	·846	44·6	0·54	·895	44·4	1·56	·732	47·0	0·84	·558	46·3	2·02	·636	45·9	8·04	·499	48·3	13·06	·505	46·3	6·98	·730	42·8	0·57
1861.	·778	47·1	2·26	·910	46·3	2·56	·802	46·6	1·62	·614	49·1	1·85	·629	51·0	8·59	·631	50·9	11·41	·798	50·0	3·57	·860	48·7	3·01
1862.	·858	47·0	5·93	·886	48·4	3·39	·722	49·9	0·88	·661	51·0	1·81	·606	52·3	7·96	·641	51·9	13·49	·726	49·9	5·59	·750	47·8	2·05
1863.	·870	47·1	0·75	·861	47·7	2·05	·837	48·4	0·92	·683	51·2	2·63	·651	51·2	9·30	·723	50·9	6·86	·841	49·2	1·61	·857	47·6	0·78
1864.	·842	45·0	3·17	·796	46·0	2·16	·578	49·8	2·34	·645	51·0	3·39	·669	51·5	6·83	·728	50·5	5·07	·707	47·8	2·03	·850	45·7	1·24
1865.	·924	48·1	5·83	·870	43·9	0·62	·655	50·8	2·44	·620	51·3	7·70	·654	52·5	9·48	·724	50·1	3·28	·721	48·4	2·12	·807	47·2	2·63
1866.	·771	48·2	0·38	·707	48·0	1·75	·666	52·9	3·77	·618	52·5	7·72	·647	51·9	17·00	·680	50·1	6·05	·754	48·1	3·07	·718	48·6	2·02
1867.	·880	44·7	3·86	·733	45·6	4·70	·694	50·9	1·83	·628	54·4	4·30	·643	52·4	11·84	·773	49·7	4·89	·897	46·8	3·09	·839	44·4	3·11
1868.	·810	47·9	1·36	·837	48·0	4·10	·938	49·9	1·04	·497	52·7	2·70	·651	54·4	12·50	·685	53·1	6·67	·812	52·5	2·32	·759	49·0	3·59

changes of solar activity upon the distribution of barometric pressure, temperature, and rainfall under different winds, the above table (p. 139) was divided into two—the first comprising the results for the five years 1858–62, when the number of solar spots was above the average, and the second those for the six years 1863–68, when the number of spots was below the average. The means of the numbers under each wind in both tables were then determined; and a comparison of the two sets of results thus obtained showed, for each element, the nature of the changes which had taken place.

Barometric Pressure.

The mean pressures under different winds for the two periods, and their differences, are as follows:—

	Mean pressure, 1858–62.	Mean pressure, 1863–68.	Difference.
	in.	in.	in.
N.	29·859	29·849	+0·010
N.E.	29·890	29·801	+0·089
E.	29·791	29·728	+0·063
S.E.	29·672	29·615	+0·057
S.	29·635	29·652	−0·017
S.W.	29·604	29·719	−0·115
W.	29·684	29·789	−0·105
N.W.	29.810	29·805	+0·005

It appears, therefore, that in the years of maximum solar-spot frequency the maximum barometric pressure took place under north-east winds, and the minimum under south-west; but in years of minimum frequency the maximum and minimum pressures occurred respectively under north and south-east winds. The difference of pressure under north-west winds is almost inappreciable; and the differences under north and south winds are small; but those under north-east, east, south-east, south-west, and west winds are too considerable to be fairly attri-

butable to accidental causes. In order, then, to determine whether they are due to the operation of a change in the intensity of solar activity, I have made the following comparison of the mean pressures under north-east, east, and south-east winds with those under south-west and west winds:—

	Mean pressure, under winds from N.E., E., and S.E.	Mean pressure, under winds from S.W. and W.	Difference.
	in.	in.	in.
1858	29·848	29·649	+·199
1859	29·813	29·672	+·141
1860	29·728	29·502	+·226
1861	29·775	29·714	+·061
1862	29·756	29·683	+·073
1863	28·794	29·782	+·012
1864	29·673	29·717	−·044
1865	29·715	29·722	−·007
1866	29·664	29·717	−·053
1867	29·685	29·835	−·150
1868	29·757	29·748	+·009

The maximum difference occurred in 1860, when solar-spot frequency was at a maximum; and the minimum difference in 1867, when solar-spot frequency was also at a minimum; and the general course of the differences has a remarkable similarity to that of the numbers representing the variations of solar-spot frequency.

As the rate of variation in the pressures during the maximum years 1858–62 was greatest in the quadrant between north-west and south-west, and as winds from the westward coming over the Atlantic are probably less affected by disturbing causes than those coming from the eastward over the continent of Europe, it appeared to me that the nature of the law of change of the pressures would be best indicated by a comparison of the differences between the pressures under north-west and south-west winds. These differences are as follows:—

1858	·221	1864	·122
1859	·238	1865	·083
1860	·231	1866	·038
1861	·229	1867	·066
1862	·109	1868	·074
1863	·134		

These numbers indicate a maximum at the end of 1859, a minimum in the latter half of 1866, and a secondary maximum at the end of 1863, thus presenting a very close agreement with the results obtained by De La Rue, Stewart, and Loewy from actual measurements of the areas of the sun-spots observed during the period under discussion.

The mean pressure under all winds is 29·744 inches in both periods; but the sum of the differences of the individual pressures from this mean is 0·755 inch in the first period, and only 0·530 inch in the second. It appears, therefore, that the forces which produce the movements of the atmosphere are more energetic in years of maximum solar activity than in years of minimum.

Temperature.

	Mean temperature, 5 years (1858–62).	Mean temperature, 6 years (1862–68).	Difference.
	°	°	°
N.	46·7	46·8	−0·1
N.E.	46·7	46·5	+0·2
E.	48·1	50·4	−2·3
S.E.	49·5	52·2	−2·7
S.	50·8	52·3	−1·5
S.W.	51·1	50·7	+0·4
W.	48·8	48·8	0·0
N.W.	46·6	47·1	−0·5

In the first period the maximum temperature occurs under winds from south-west, and in the second period under winds from about south-south-east. The greatest differences between the two periods occur with east, south-east and south winds. Comparing the mean temperature

under south-west winds with that under south and south-east winds we have the following differences :—

	°		°
1858	+0·35	1864	−0·75
1859	+0·75	1865	−1·80
1860	+2·20	1866	−2·10
1861	+0·85	1867	−3·70
1862	+0·25	1868	−0·45
1863	−0·30		

Here we have again a maximum in 1860 and a minimum in 1867.

As the temperature diminished under two winds only (the north-east and south-west), we may compare the means of the temperatures under these winds with those of the wind under which the greatest increase of temperature took place, the south-east, thus :—

	°		°
1858	−1·15	1864	−2·75
1859	−0·75	1865	−4·30
1860	+0·05	1866	−3·45
1861	−0·50	1867	−6·75
1862	−0·85	1868	−2·15
1863	−1·90		

Again we have a maximum in 1860 and a minimum in 1867; and it is therefore evident that the distribution of temperature under different winds, like that of barometric pressure, is very sensibly influenced by the changes which take place in solar activity.

Rainfall.

	Mean annual amount.		Difference.
	5 years (1858–62).	6 years (1863–68).	
	in.	in.	in.
N.	3·16	2·56	+0·60
N.E.	3·33	2·56	+0·77
E.	2·23	2·06	+0·17
S.E.	2·30	4·74	−2·44
S.	7·70	11·16	−3·46
S.W.	11·51	5·47	+6·04
W.	4·73	2·37	+2·36
N.W.	1·75	2·23	−0·48

In the first period the maximum fall occurs with south-west, and in the second period with south winds; and the greatest differences between the two periods are with winds from south-east, south, south-west, and west, the differences with south-east and south winds being negative, and those with south-west and west winds positive. Comparing, then, the sums of the amounts which fell under the first two winds with those which fell under the last two, we have the following results:—

	Sum of S.E. and S.	Sum of S.W. and W.	Difference.
	in.	in.	in.
1858	6·32	11·53	− 5·21
1859	13·42	15·58	− 2·16
1860	10·06	20·04	− 9·98
1861	10·44	14·98	− 4·54
1862	9·77	19·08	− 9·31
1863	11·93	8·47	+ 3·46
1864	10·22	7·10	+ 3·12
1865	17·18	5·40	+11·78
1866	24·72	9·12	+15·60
1867	16·14	7·98	+ 8·16
1868	15·20	8·99	+ 6·21

It will be observed that in every year of the first period (1858–62) the differences were negative, while in every year of the second period (1863-68) they were positive—or, that the amounts of rainfall under south-west and west winds were invariably greater than those under south-east and south winds during the years when the number of solar spots was above the average, and invariably less in the years when the number of sun-spots was below the average,—and, further, that the greatest difference in the first series of years occurred in 1860, at the time of a solar-spot maximum, and that in the second series in 1866, at or very near the time of a solar-spot minimum.

Considering the irregular character of rainfall, both in the times of its occurrence and the amounts in which it

falls, I confess I was scarcely prepared to expect that the results of rainfall observations would agree so closely with those of barometric pressure and temperature.

Instead of comparing the differences between the amounts of rainfall, it would perhaps be more correct to compare their ratios; but the results would be substantially the same. Thus, dividing the entire series of 11 years into three groups—the first including the four years 1858–61 (one of which was a year of maximum frequency of solar spots), the second the four years 1862–65, and the third the three years 1866–68 (one of which was a year of minimum frequency)—we have the following amounts and their ratios :—

	Sum of rainfall under S.E. and S. winds.	Sum of rainfall under S.W. and W. winds.	Ratio.
	in.	in.	
4 years (1858–61)	40·24	62·13	0·64
4 years (1862–65)	49·10	40·05	1·22
3 years (1866–68)	56·06	26·09	2·14

Here we have a small ratio in years of maximum solar activity, and a large ratio in years of minimum, and a ratio of intermediate value for the intervening years.

It will, I think, be admitted that the results of this investigation support very strongly the hypothesis which led me to undertake it. They show also strikingly that the future progress of meteorology must depend to a much greater extent than has been generally supposed upon the knowledge we may obtain of the nature and extent of the changes which are constantly taking place on the surface of the sun; and therefore, in the interests of meteorological science, it is evidently very desirable that observations of solar phenomena should be greatly multiplied, by the establishment, in various parts of the world, of observatories specially devoted to this object, so that a con-

tinuous daily, or even hourly, record may be obtained of the state of the solar disk and its appendages, and the results discussed in connexion with those of observations of meteorological phenomena.

XVII. *On the Distribution of Rainfall under different Winds, at St. Petersburg, during a Solar-spot Period.*

By Joseph Baxendell, F.R.A.S.

Read before the Physical and Mathematical Section, December 5, 1871.

In the paper which I read at the last meeting of the Section it was shown that, at Oxford, changes take place in the relative amounts of rainfall under different winds in a period corresponding with that of solar-spot frequency. Thus, in the years when the number of groups of solar spots, as observed by Schwabe, was above the average, the amount of rainfall under west and south-west winds was greater than that under south and south-east winds, while in the years when the number of groups of solar spots was below the average the reverse of this took place, the amount of rainfall under west and south-west winds being *less* than that under south and south-east winds. The hypothesis which led to the investigation requires, however, that great diversity should exist in the relative amounts of rainfall under different winds at different stations. While at some the distribution will be similar to that at Oxford, at others it will be of an opposite, and in others again of an intermediate character; but, whatever may be the nature of the distribution at any station, the changes to which it will be subject will take place in a period identical with the solar-spot period. In some localities the changes will be so slight, or so irregular, as not to be immediately referable

to any well-defined law. These points on the surface of the earth may be regarded as nodal points in the general system of circulation of the great currents of the atmosphere.

Among the places at which it seemed to me likely that the law of change in the relative amounts of rainfall under different winds would be found to differ considerably from that which prevails at Oxford is St. Petersburg. I therefore extracted from the volumes of the 'Annales de l'Observatoire Physique Central de Russie' the amounts of rain which fell under different winds at St. Petersburg during the eleven years 1854–64. The results are shown in the following table:—

Rainfall under different Winds, at St. Petersburg, during a Solar-spot Period.

Year.	N.	N.W.	W.	S.W.	S.	S.E.	E.	N.E.	Calm.
	in.	in.	in.	in.	in.	in.	in.	in.	in.
1854.	0·800	0·675	3·543	2·101	1·088	0·776	1·041	1·087	1·644
1855.	2·056	2·688	1·192	3·688	1·720	0·558	1·509	0·961	1·325
1856.	0·313	1·014	5·174	2·331	1·386	1·551	0·535	1·852	0·800
1857.	1·871	0·000	2·700	1·640	1·223	0·757	0·181	2·518	1·856
1858.	0·213	0·445	2·218	2·441	0·475	2·759	1·025	1·075	1·002
1859.	0·375	0·548	4·961	4·371	2·329	2·251	1·038	0·618	0·639
1860.	1·400	1·182	2·194	3·088	1·910	2·460	2·469	0·301	0·683
1861.	1·861	0·123	6·327	2·681	3·225	2·259	1·376	0·978	0·332
1862.	1·045	1·448	3·290	2·717	2·032	1·921	0·497	0·431	0·368
1863.	0·332	2·446	2·521	3·390	3·110	1·984	0·512	0·831	0·000
1864.	2·171	6·560	3·038	4·580	2·017	7·532	1·201	2·430	0·656
Means	1·131	1·557	3·378	3·002	1·865	2·258	1·035	1·189	0·846

From the mean values in the last line of this table it appears that there was a principal maximum of rainfall under west winds, and a secondary maximum under south-east winds, a principal minimum under east winds, and a secondary minimum under south winds.

In the eleven years 1854–64 the number of groups of solar spots, as observed by Schwabe and others, was above the average in the five years 1858–62, and below the average in the remaining six years 1854–57 and 1863–64.

I therefore divided the series of rainfall-results into two corresponding series; and taking the means of the amounts under each wind, I obtained the following numbers:—

	Mean annual amount of rainfall, 1858–62. in.	Mean annual amount of rainfall, 1854–57 and 1863–64. in.	Difference. in.
N.	0·979	1·257	−0·278
N.W.	0·749	2·230	−1·481
W.	3·798	3·028	+0·770
S.W.	3·059	2·955	+0·104
S.	1·994	1·757	+0·237
S.E.	2·330	2·198	+0·132
E.	1·281	0·830	+0·451
N.E.	0·681	1·613	−0·932
C.	0·605	1·047	−0·442

The differences in the last column show that the mean amounts of rainfall under west, south-west, south, south-east, and east winds are greater in years of maximum solar-spot frequency than in years of minimum, while the amounts under north-east, north, and north-west winds, and calms, are less. Comparing, then, the total amounts which fell under west, south-west, south, south-east, and east winds in each year with those which fell under north-east, north, and north-west winds, and in calms, we have the following results:—

	Total amounts of rainfall under W., S.W., S., S.E., and E. winds. in.	Total amounts of rainfall under N.E., N., & N.W. winds and calms. in.	Ratios.	Corrected ratios.	Groups of solar spots.
1854	8·552	4·026	2·03		
1855	8·697	7·030	1·23	2·00	79
1856	10·977	3·979	2·75	1·67	34
1857	6·501	6·245	1·04	2·34	98
1858	8·918	2·735	3·22	3·71	188
1859	14·950	2·180	6·86	4·49	205
1860	12·121	3·566	3·39	5·00	211
1861	15·868	3·294	4·76	3·77	204
1862	10·457	3·292	3·17	3·71	160
1863	11·517	3·609	3·19	2·64	124
1864	18·368	11·816	1·55		

The mean ratio is 3·01; and the ratios for the years of maximum solar-spot frequency are all above this mean, while those for minimum years are all below it, with only one unimportant exception.

In order now to eliminate as far as possible the effects of accidental disturbing causes, we may take the means of the ratios of every three successive years; and in this way we obtain the corrected ratios in the fifth column of the above table. For convenience of comparison I have added in the sixth column the number of groups of solar spots observed in each year by Schwabe; and a glance at the two sets of numbers will show the remarkably close agreement which exists between them in the times of their maxima and minima, which seems to me fully to justify the conclusion that both classes of phenomena are intimately connected, either as cause and effect, or as effects of the same cause.

Excluding the amounts of rain which fell during calms, the corrected ratios become:—

1855	2·77	1860	6·42
1856	2·15	1861	4·37
1857	3·32	1862	4·04
1858	5·40	1863	2·80
1859	6·31		

It will be observed that the course of these numbers is almost identical with that of the numbers obtained when the amounts of rain which fell during calms are combined with those which fell under north-east, east, and north-west winds.

The close agreement which has thus been shown to exist at St. Petersburg between the times of maximum and minimum frequency of solar spots, and those of the variations in the distribution of rainfall under different winds, gives increased value to the results derived from the Oxford observations, and affords additional support to the hypo-

thesis I ventured to advance in a former paper—that changes in solar activity, and consequently in the magnetic condition of the earth, produced corresponding changes in the directions and velocities of the great currents of the atmosphere, and in the distribution of barometric pressure, temperature, and rainfall. It is therefore evidently very desirable to discuss observations made at stations in various parts of the globe with reference to the variations which take place in solar activity, and thus to determine for each station the nature of the changes which take place in the relations between the several meteorological elements during a solar-spot period.

XVIII. *On a peculiar Fog seen in Iceland, and on Vesicular Vapour.* By R. Angus Smith, Ph.D., F.R.S., &c.

Read October 29th, 1872.

One bright afternoon in July a party from Mr. Young's yacht (the 'Nyanza') landed in Reikjavik and took tea at a hospitable house in that capital. After tea we were invited to go out and see the fashionable people on the promenade. We saw few people; but, as soon as we left the house, a cloud came down a street from southwards, and some one said "let us cross out of the way of the dust." Dust was strange in such a place, where there is gravel enough, but few particles on the ground so fine as to be called dust. I looked more carefully, and, finding the cloud moving very slowly on the ground, concluded that it was smoke from a chimney, but smoke

mixed with larger particles than we generally see, caused probably by the excess of ash driven up the chimney. Gradually it came on to us; there was no smell, but a distinct chill. "It is not smoke; it is fog." We went up the rising ground that leads to the tower on the great inland road, and we saw the fog rising out of the small lake or pond behind the town and rolling into the streets very slowly. As this advanced, a similar fog rose from the sea and rolled also into the town, so that the two met in the streets. It is clear, therefore, that wind had nothing to do with the matter, but that both rolled from the fact that they were too heavy to remain suspended. I will not say that this is uncommon; but I will say that it was accompanied here by a larger size of particle than I have ever seen, and that the flatness with which it fell on the ground and the lumbering mode of rolling distinguished it from all fogs which I have seen. It was not even common there, because the people took it for dust; and dust it certainly looked like, in part because the whole body of the fog was not composed, I believe, of particles equally large; had it been so, it would probably have been more transparent. I have seen the fogs of great cities at their worst, and have watched fogs and clouds on the plains and hills, and been enveloped in both so often as to make many of their phenomena familiar (I cannot say all, because there seems to be no limit to their variations of appearance); but I have not at any time seen any approach to the strange sight at Reikjavik. After standing on the hill and looking at the fog, with the chimneys of the small town standing out of it, we came down; but the air had become clear. The fog lasted only about an hour and a half.

On examining the particles with the aid of a magnifier, somewhat of the same power as that used by Saussure when he made the observations to be spoken of, I con-

sidered that they were in diameter about $\frac{1}{400}$ to $\frac{1}{300}$ of an inch. This could not be determined at the time; but I remembered the size very well, and measured a body which I considered about the same when I came home. I also obtained the particles spoken of by Saussure exactly in the way he obtained them, and came to the same conclusion as to their size, but was equally certain that those of the fog were entirely different, being at least ten times as much in diameter. These particles, when seen through a glass, were perfectly spherical, and seemed quite concrete, as Saussure would say, or solid, as some people would remark—although this word has a different meaning when applied to water; in other terms, they seemed to be similar throughout, without any hollow centre. The sun was not down, the sky was still brilliant; and indeed this brilliancy and the colour of the clouds towards the occident did not leave as late as $11\frac{1}{2}$ o'clock, and I do not see any reason to suppose that it left at all; it probably was seen till the sun rose. The light was reflected from some of the spheres in the extreme of the fog, and from some not; but all seemed concrete, and, being transparent, had a very beautiful appearance. They all tended downwards; they were falling evidently; it was a falling dew, or a slight incipient rain, rapidly disappearing into the earth. A few globules of air might be entangled in the vapour—of course without systematic vesiculation, a most difficult thing to imagine. It seemed evident to me that to make a distinction absolute between fog, rain, and dew was a waste of words. There is a broad observable distinction, but no narrow line; and we cannot tell the end or beginning of either.

Holding that the opinion regarding vesicles rests on a foundation too weak to be worth much attention, I do not consider it necessary to examine the history of the subject with great minuteness. However, as it is not well known,

and much disregarded in works on Meteorology, I shall give the opinions of some who are the principal originators or at least those best known to me. To begin with Edmund Halley, the astronomer, I quote from the abstract of his paper in the 'Philosophical Transactions.' The actual paper itself is not much longer, but it was not at hand.

"I have formerly attempted to explain the manner of the rising of vapour by warmth, by showing that if an atom of water were expanded into a shell or bubble so as to be ten times as large in diameter as when it was water, such an atom would become specifically lighter than air, and rise as long as that flatus or warm spirit that first separated it from the mass of water shall continue to distend it to the same degree; but that warmth decreasing, and the air growing cooler and so specifically lighter [heavier is meant], the vapours consequently will stop at a certain region of the air or else descend, which may happen on several accounts, as will be seen below. Yet I assert not that this is the only principle of the rise of vapours and that there may not be a certain kind of matter whose conatus may not be contrary to that of gravity, as is evident in vegetation, wherein the tendency of the sprouts is directly upwards, or against the perpendicular. But whatever be the true cause, it is certain that warmth does separate the particles of water and emit them with a greater velocity as the heat is more intense, as is evident in the steam of a boiling caldron, wherein, likewise, the velocity of the ascent of the vapours visibly decreases till they disappear, being dispersed and assimilated into the circumambient air."—*Phil. Trans.* (abridged) vol. iii. p. 428.

Gottlieb Kratzenstein, candidate in Medicine at Halle, obtained the prize from the Bordeaux Royal Academy of Belles-Lettres, Sciences, and Arts, for his 'Théorie de

l'Élévation des Vapeurs et des Exhalaisons, démontrée mathématiquement,' published at Bordeaux in 1743. This and the next mentioned are written in Latin and French.

He says:—"IV. I put a vessel of boiling water at a window, so as to have more light. I observed two kinds of watery particles rising from it: the first, of a white colour, dividing into different parts, remained for some seconds on the surface; and then I saw one division after another separate from the surface and rise into the air with an accelerated movement. 2. Whilst the water boiled, the vapour moved about two feet and a half in three seconds. 3. Amongst the vapours I saw another kind—small transparent globules, not hollow—which rose very quickly about a foot high; but their motion was uniformly retarded, and they described a parabolic curve in descending.

"V. The principles of mechanics teach us that bodies which describe a parabolic curve are pushed by a violent force; it follows that the second kind of particles are pushed out of the water and rise by a violent impulsion.

"VI. Since this class of particles falls back, these cannot be called vapour.

"VII. Since the little particles keep a spherical form in the midst of the air by reason of the equal adhesion of their parts, the vapours ought also to have a spherical form."

"X. I exposed a vessel of boiling water to the rays of the sun, which entered by an aperture into a chamber. I placed myself in such a way that my eye made an angle of 42 degrees with the falling ray; and I was unable to see in the vapours the colours of the rainbow. To be more certain, I caused a jet of water or fine rain to fall into the middle of the vapours; and having placed myself as was required, I saw two segments of a rainbow in the middle of the vapours—that is to say, a segment of the principal bow and a segment of that of which the colours

are reversed and less vivid; but as soon as I removed the jet they both disappeared."

"XII. The colours came from the reflection and refraction of the sun's rays, in a certain manner, in the drops of water; it follows that this must take place differently in vapours and in drops of water not hollow. Now there can be no other cause of this difference than the interior figure, because the exterior is the same. Therefore the vapours are hollow vesicles."

He then found that, in a globe with water, when the air was compressed he saw no vapour till he made an opening to the air, when the vapour became visible and the colours of the rainbow appeared. He also formed visible vapours by pumping the air out of a globe with aqueous vapour. Some particles he found different from others, and twelve times as fine as a hair; the diameter of the hair was the three-hundredth part of an inch, and of the vapour particles $\frac{1}{3600}$.

He concludes, from the experiment by exhaustion, that the vapours which keep themselves in the air naturally are invisible, and become visible when they descend and expand.

He considers that the rays are refracted from the vapours as from soap-bubbles. To the objection that the refraction may be in the concrete bubble, he replies that "some vapours show an order of colours very different from others; vapours are, therefore, of different sizes. They do not reflect colour from their whole volume."

To reflect colours in the order seen, he says the pellicle of the vapours must be very fine. Then he wishes to show that air is necessary to keep up vapours, because if the air is pumped out the vapours become visible.

He thinks the elastic fluid causing ebullition in a vacuum goes through the pores of the vessel.

He sees that vapours have no absolute lightness, and cannot lose their specific gravity.

He also says (in XL.), "The specific weights of water and air are as 900 to 1. A vesicle of water must therefore be a thousand times as large as the drop of water of which it is composed. The vapours, therefore, cannot be so dilated as to rise by being specifically lighter than air."

But how are these vesicles elevated?

"According to Mr. Halley's observations, air which touches boiling water is dilated one quarter, and the air heated by the sun is dilated by a seventh. Therefore the air becomes lighter than the colder air which surrounds it; it ought, therefore, to rise with an accelerated movement, and to employ a quarter of its weight as force." "The bubbles are not able to overcome by their weight the cohesion of the air, but remain suspended."—LX. 4.

"The vapour will rise with the air when it preserves its heat; the air would lose its heat very slowly, because the cold air which surrounds it has a very small specific gravity."—LX. 6.

"The motion of the air ceasing, the molecules of the air remain adhering to the vapours; and because the vapours, being so small, are unable by their weight to overcome this adherence, they become supported in the air even when it is calm."—LX. 7.

I think I have shown enough from M. Kratzenstein to prove that he has given in reality no argument for the vesicularity of vapours. He has, in fact, shown quite clearly that the vesicularity does not do away with the difficulty of floating water in the air; and he has taken refuge in heated and expanded air. Not knowing that the cold produced by expansion allows the vapours to be seen, he is far misled, and ends at last in the cohesion of the air and smallness of the particles, forgetting apparently the importance at first given to his vesicles. The whole is like the work of a clever youth, as the author seems to have been. By diminishing the size of the par-

ticles he may, it is true, diminish to an indefinite extent the resistance of the air; but the heat, which he wishes to retain, cannot be allowed to him.

In the same year, and at the same Academy of Bordeaux, a prize was adjudged to M. Hamberger, Professor of Physics and Medicine in the Academy of Jena. Both he and Kratzenstein were Germans, and from Saxony. Hamberger shows that the weight of water cannot be reduced by increasing the size of the vesicles or, indeed, by making vesicles. He says, "the force which raises bodies into the air diminishes with the size of the bodies; but as the elevating forces act only on the surface, and these diminish as the squares of their diameters, whilst the bodies (and consequently their weight) diminish as the cubes, it is easy to see that a force insufficient to raise a body may raise the small portions." In other words, the smaller the body the less can it resist the air. It is somewhat wearisome to go over the eighty-four paragraphs, some considerably divided; so I need only add one or two.

"71. Nothing but the motion of the air is the ultimate cause of the rise of vapours. We must now therefore explain what is the cause which, during the time of evaporation, produces the motion of the air."

"72. The movement of the air and its direction upwards are produced by the vapours themselves, which are heated when raised into the air by causes explained in par. 63, no. 2, letter B, and no. 3, letters A and B, because the heat passes from the vapours warmed into the air which touches the vapours, whence it arises that this air is heated and expanded; consequently it becomes specifically lighter than the surrounding air, by which it is pressed upwards. This air follows the dilated air, which yields its place and, being put in motion upwards, carries with it the vapours attached to it."

It is clear that, in this, Hamberger has found a possible way of raising vapours and of keeping them afloat; whereas the others have wandered more amongst vapours heated internally, which we know must soon equalize their heat; heat-particles have also led some astray.

We may, in reality, say that amongst those who seem to have been the founders of this theory of vesicles, no reasons have been given, but only a few fancies by no means well supported even in the opinion of the authors, and still less in that of natural philosophers at the present time.

We may conclude that a drop of water may be rendered more easily supported and moved in the air by two methods easily imagined:—the first, namely, by increasing the size in the vesicular method; the second, by being broken into minute fragments. In clouds we may suppose either cause; but no reason is given for the first, and we must fall back on the second. These fragments are of various sizes.

I have not searched carefully the authors who have taken up the subject of vesicles, and do not think it well to spend much time upon it; but Horace-Bénédict de Saussure, being the most notable, and the one to whom the idea itself is generally referred, must be carefully attended to. In his 'Essais sur l'Hygrométrie' there is a chapter on "Vesicular and Concrete Vapours," at page 282. A few of the most characteristic sentences will be given.

Par. 199. "If there is no body contiguous to that air which is supersaturated with vapour, so that it may attach itself, the elements of water unite together and form either small spherical full drops, or small congealed needles, or, finally, hollow spheres."

200. "These little solid drops, which unite to form rain and the frozen needles (the first rudiments of snow), may be considered not to be vapours; but as they are so fine as to

remain suspended a long time in the air and cause other meteoric results, I think it better to leave them under the head of vapours, and to call them *concrete vapour*."

201. "As to the hollow spheres, which I have called vesicular, it appears that conjecture has been in advance of observation; and their existence has been supposed in order to account for the formation of vapour before knowing that they could be made to fall before the senses. Desaguiliers, in his 'Cours de Physique Expérimentale,' tom. ii. leçon x., opposes, by abstract reasoning, the physicists of his time who admitted the existence of vesicles, and endeavours to refute this opinion as a purely gratuitous hypothesis, or at least one which does not depend on direct observation. One may now make these vesicles visible to the least-experienced eyes.

"This I consider the best way to observe them. Expose to the rays of the sun, or at least to full daylight and where the air is still, a cup of very hot water of a black or dark colour—for example, coffee, or water mixed with ink. From this will arise fumes more or less dense, which will rise for a little and disappear. An observant eye will perceive that this vapour is composed of rounded granules, whitish and detached. But to see them better, we must use a lens of an inch or an inch-and-a-half focus; and with this the surface of the liquid ought to be observed; but care must be taken to hold the lens out of the current of the rising vapours, as they impair its transparency.

"Observing attentively what passes on the surface of the liquid, we see little spherical balls of different sizes, and moving with different rapidities. The freest rise rapidly and vanish; but the coarser fall back into the cup without mixing with the liquid from which they escaped, and roll on the surface like light dust, and so on. . . . . It is sufficient to see them to be convinced that these are hollow spheres."

He then tells of Kratzenstein's measurements, and resumes at 203 :—"This is my mode of observation. I have a sort of eolipile made with two bulbs—a tube of glass sealed at A and open at D; the two bulbs C and B communicate with each other and the opening at the point D. I pass a few drops of water into the bulb B, and put it on the flame of a spirit-lamp. I use spirits of wine, so as not to soil the bulbs of the eolipile. As soon as the water is sufficiently heated in the bulb B, and the bulb C cold, one sees the vapours coming out of B, enter into C, and condense in the form of a cloud which is entirely formed of vesicles. But when the water begins to boil in B, the torrent of elastic vapours which enter C reheats it; the vapours no longer condense; we see clouds or vesicles no longer. But take away the eolipile from the flame, and with a little fresh water cool the bulb C; one sees then immediately a vesicular vapour. Under a microscope these vapours may be better observed; but the rapidity of their movements prevents us seeing them with large magnifiers."

At p. 293, par. 206, he speaks of clouds, and says again "This is my mode of observation;" but it ends simply in looking at the fog with a lens, and waiting till he sees a drop or a particle pass.

Finding that the thinness of the envelope will not give them qualities sufficient, he, as others before him, seeks some other liquid, and in par. 210, talking of an atmosphere rarer than air surrounding bodies, says, "Even our vesicles give a very striking proof of the existence of their atmosphere, and that by the ease with which they roll on the surface of water without mixing or adhering; for it is evident that if they adhered they would be attracted strongly." But this is another subject.

Par. 211. "Is this fluid electricity?"

In par. 213, speaking of the cause of vesicularity, "We have no distinct notions of the intimate and elementary

structure of bodies. I will say only that most liquids have a marked disposition to take this form, and that it appears to be the result of their viscosity or mutual attraction of their elementary parts and of the figure proper to those parts; it is a kind of crystallization—the degree which water is capable of whilst it retains sufficient heat to be fluid."

The conclusions drawn by Saussure are quite unwarranted by his observations; and one can scarcely find anywhere that he sees any thing that would lead him to consider the particles hollow. It is a vague inference from certain of their qualities and their supposed analogy to soap-bubbles.

I have tried his few experiments, and think he may be right about the size of the granules he speaks of when water condenses in a bulb; but as to their vesicularity, I consider he has no right to pronounce an opinion in this case. If vesicularity be proved, it must be by larger powers than he has used, or by better eyes than he pretended to have. If there are vesicles formed by condensation, they are still to find. And this will apply either to the vapour in the glass bulb or the atmosphere above us.

When vapour rises from water which is heated, or, let us say, in ebullition—on bubbling, from whatever cause, the result may be different. If steam raises a bubble of water, it may take it into the air, and on cooling leave a little air which it may have received from the water. The amount in this case will be excessively small; but I do not know that it has ever been observed. If, however, gas should be present in greater quantities than the air in water, we may expect a larger bubble to remain formed for a while. This is especially the case when hydrogen bubbles out of an acid solution where zinc or iron is dissolving. It seems to form small balloons which carry the

salt to a great distance. I have sometimes, in this case, believed that I saw the proof of hollowness of such vesicles; and the great range of their flight is remarkable to any one who will stand even in a large room where metal is dissolving. I have sometimes found branches of trees, set a considerable distance from works where sulphate of iron was manufactured, so covered with crystals as to be striking to passers-by. This observation of the balloon-forming character in the evolution of hydrogen gas was first made to me by my friend James Higgin, F.C.S.

This observation, however, is entirely independent of any thing that we know to occur in the regions of the air where cloud is formed. The remarkable vapour which was described at the beginning, and which led me to look a little into this subject, showed me no signs of vesicularity where one would have supposed there existed the best opportunity of seeing it.

That there are difficulties attendant on the explanations of the subsistence of vapour, it is easy to see; but vesicles of any kind we can suppose must be heavier than the air around them, and even the inventors have been aware of the new difficulty, and sought to lift them up by various means. They have probably arisen out of the imagination of people active-minded and speculative, and been seen by the eyes of persons whose reasonings were beforehand affected with the notion. If vesicles should be found (and who knows what wonderful things there may be found in the future?), the discovery must have an entirely new origin and basis.

Indeed I see no reason for going far for a mode of keeping clouds up. Times out of number I have observed on calm summer evenings a cloud of smoke from a steamboat-funnel, lying for miles in length at a height very little different from that of the funnel out of which it issued. Sometimes this will be found a little lower. At other

times I have found the smell of a cigar, used by a person fully a quarter of a mile on, over the road at about the same height as his mouth, nothing being visible. In these cases have we any thing to look to but the size of the particles? They are so small that their resistance to the atmosphere is diminished to its utmost, as the resistance of the air is increased so much in proportion to the weight that they cannot fall rapidly.

We can, then, make clouds lie on the air without falling. In some cases these artificial clouds rise; this is no doubt caused by upward currents, which must exist for various reasons. One very common is that when sailing you come to the end of the current of air or wind very suddenly; it goes neither to the right nor left. These currents make long serpentine movements in the line of smoke, and abundantly explain alternate calms and winds on one line, so often observed by sailors.

If, however, the particles of the water forming a cloud are large, there is abundant tendency to fall observed, as in other bodies. Dust from a chimney will fall down on a deck at once; but the smoke will float, as said. Let the particles be large, and the fall will be rapid and evident. If, as in the case of the fog observed at Reikjavik, the particles are inordinately large, the fall is also greater than usual; the water acts exactly as dust in similar cases acts, with due respect to specific gravity and other qualities. One of these is the fact that, unlike dust, the fine vapour-particle will evaporate and become transparent if heat is given. When, then, it falls a little, in certain atmospheres it is resolved into gas, as we may say; but in this state it may rise and be again cooled, and so on without intermission; and this may be one of the modes of keeping clouds pretty uniform in the air. It is, however, to be observed that they are never long of the same appearance; they are constantly changing; and no

doubt this unforming and reforming is very active at times. Sometimes the unforming is seen clearly in a constant fall of vapour which never goes below a certain level, sometimes in rising up a hill to one definite point. A very curious and frequent occurrence is the formation of vapour from a current of air as soon as it touches a peak, and the complete evaporation of all as soon as it leaves the peak a few hundred yards—this going on for hours, a constant forming and reforming. So much is this the case that I thought of assigning it as a reason for the suspension generally of all clouds; but I do not see it acting on all occasions; and at any rate the fact of the cloud of carbon gave an analogy which showed that size was not to be forgotten in the matter. I can imagine that size in the vapour of carbon from chimneys is not the only point, but that the form of the carbon, light and feathery, and so holding air, may diminish the specific gravity; but, as in the case of vesicles, it can never reduce the gravity to that of the air. Besides, we see the matter illustrated by dust, with equal precision, although, so far as I have observed, not in such extreme cases. We see an example also in sulphuric acid and sulphur-fumes mixed with various substances, arsenic &c., in copper-smoke. We see the same in the smoke from iron-furnaces sending out lime and other substances, which go like clouds to great distances. Indeed the matter is illustrated in nature to extremes, and any one can find out some point for himself.

Of course we cannot deny the possibility of the air being entangled in the condensed vapour and so assisting; but how enormous must be the proportional amount of air to keep the whole suspended? Even the hydrogen little balloons spoken of fall soon, although that light gas supports them better than common air can do.

XIX. *On Black-bulb Solar-Radiation Thermometers exposed in various Media.* By G. V. Vernon, F.R.A.S., F.M.S.

Read before the Physical and Mathematical Section, February 27th, 1872.

Being desirous to make some comparisons of the readings of black-bulb thermometers exposed in various media, I got Messrs. Negretti & Zambra to make me a set of three thermometers, in addition to the ordinary black-bulb maximum *in vacuo*.

The glass tubes containing the thermometers were filled with hydrogen gas, carbonic-acid gas, and atmospheric air, at 32° F., the latter thermometer being described in the tables as filled with compressed air. The instruments were all alike, the glass tubes enclosing them being of equal thickness. The thermometers were all compared with the Greenwich standard, and require no index-error correction.

The observations were made in the years 1861 to 1865; and the period embraced was just four years. Since the latter year the observations have been discontinued; but the thermometers remain in the same position they were originally placed in.

In the annexed Tables, Table I. gives the mean monthly readings of the thermometers for each year, with the additional readings of the black-bulb freely exposed and the maximum thermometer in the shade.

Looking at the yearly means, the black-bulb *in vacuo* gives the highest mean reading, the one with carbonic-acid gas comes next, followed by the condensed-air one, that filled with hydrogen giving the lowest temperature.

Examination of the monthly values shows that the maxima for all the thermometers occur in July, and the

minima in January. The minima of the enclosed thermometers read nearly all alike; the maxima of the *vacuo* and carbonic-acid ones are nearly equal; and the same remark applies to the hydrogen one and the one filled with compressed air: the latter agrees with what Tyndall points out, that hydrogen and atmospheric air absorb heat equally.

Table III. gives the difference of each monthly mean referred to the reading of a freely exposed black-bulb thermometer.

In volume v. page 169, of Symons's 'Meteorological Magazine,' there is a paper by Mr. Francis Nunes, giving comparisons of carefully made black-bulb thermometers by Pastorelli, showing a considerable difference between the thermometer *in vacuo* and the one partially exhausted; his observations were made in October, and show a difference of 1°·2 to 11°·5, the *vacuo* thermometer being the highest of the two. Mr. Nunes also states that an enclosed thermometer without any exhaustion reads still lower, being from 0°·8 to 12°·8 below the *vacuo* thermometer.

From my observations the difference between the *vacuo* and condensed-air thermometers is never very large, amounting rarely in individual cases to 5°·0 to 6°·0, but in July 1865 reached occasionally 10°·0, the mean difference in July only reaching 4°·3.

I am not aware of any similar series of observations to be found anywhere else, and thought it might be desirable to tabulate the values for comparison with any subsequent series that may be made.

TABLE I.—Radiation Thermometers. Mean Monthly Maximum in the Sun.

Year.	*In vacuo.*	In carbonic-acid gas.	In hydrogen gas.	Condensed air.	Black-bulb freely exposed.	Maximum in shade.
			JANUARY.			
1862.	48°0	46°3	46°4	47°8	44°6	43°2
1863.	.....	.....	.....	.....	.....	.....
1864.	45°1	45°0	44°7	45°4	43°4	41°9
1865.	44°3	44°1	44°1	44°2	43°6	41°3
Means	45°8	45°1	45°1	45°8	43°8	42°1
			FEBRUARY.			
1862.	54°8	53°2	53°0	54°2	49°7	46°3
1863.	65°0	61°9	62°4	62°1	55°2	50°0
1864.	50°5	49°3	48°9	53°6	45°5	42°0
1865.	50°7	51°3	51°1	50°8	46°8	42°3
Means	55°2	53°9	53°8	55°2	49°3	45°1
			MARCH.			
1862.	64°4	61°3	60°3	60°7	56°8	48°9
1863.	75°7	73°6	72°1	72°1	62°8	52°6
1864.	70°6	69°6	68°3	69°1	59°8	48°9
1865.	65°3	64°9	65°9	63°1	57°0	44°1
Means	69°0	67°3	66°6	66°2	59°1	48°6
			APRIL.			
1862.	81°6	76°1	73°6	75°3	70°8	57°5
1863.	86°0	83°3	81°2	81°8	69°9	56°2
1864.	85°8	83°4	81°9	82°8	76°0	60°0
1865.	92°6	91°3	89°4	89°3	81°5	63°5
Means	86°5	83°5	81°5	82°3	74°6	59°3
			MAY.			
1862.	95°7	89°1	86°9	86°3	81°2	64°4
1863.	88°4	87°1	86°1	86°6	74°8	61°2
1864.	97°5	96°4	95°0	95°8	81°4	66°5
1865.	93°7	88°2	87°1	87°9	79°3	65°0
Means	93°8	90°2	88°8	89°1	79°2	64°3

TABLE I.—Radiation Thermometers. Mean Monthly Maximum in the Sun (*continued*).

Year.	*In vacuo.*	In carbonic-acid gas.	In hydrogen gas.	Condensed air.	Black-bulb freely exposed.	Maximum in shade.
			JUNE.			
	°	°	°	°	°	°
1862.	88·9	86·3	84·2	84·8	69·6	58.8
1863.	97·2	96·6	93·0	94·1	81·4	66·8
1864.	98·9	97·5	94·9	95·5	81·2	66·2
1865.	101·8	104·6	99·0	101·0	93·0	72·8
Means	96·7	96·2	92·8	93·8	81·3	66·1
			JULY.			
1862.	97·8	94·0	91·9	91·8	80·3	66·7
1863.	101·1	100·7	98·1	98·7	87·3	70·8
1864.	100·8	108·6	98·8	98·8	86·0	70·9
1865.	110·6	107·2	103·4	103·8	94·4	76·6
Means	102·6	102·6	98·1	98·3	87·0	71·2
			AUGUST.			
1862.	.....	.....	.....	.....	.....	.....
1863.	93·9	93·5	91·8	92·7	83·4	68·7
1864.	96·3	94·5	91·0	92·1	80·9	68·0
1865.	102·6	97·0	94·8	97·0	81·7	68·7
Means	97·6	95·0	92·5	93·9	82·0	68·4
			SEPTEMBER.			
1861.	86·5	80·9	78·5	79·6	78·5	63·9
1862.	83·6	81·6	80·0	80·0	74·2	62·6
1863.	79·2	78·5	75·2	78·7	70·1	58·5
1864.	91·7	88·4	84·6	90·1	73·3	65·2
Means	85·2	82·3	79·6	82·1	74·0	62·6
			OCTOBER.			
1861.	72·6	69·7	68·9	70·9	64·8	60·6
1862.	71·3	68·0	66·6	67·2	61·9	56·5
1863.	66·0	66·2	64·6	67·5	62·0	55·9
1864.	69·3	66·9	64·9	68·6	61·7	57·0
Means	69·8	67·7	66·2	68·6	62·6	57·5

TABLE I.—Radiation Thermometers. Mean Monthly Maximum in the Sun (*continued*).

Year.	*In vacuo.*	In carbonic-acid gas.	In hydrogen gas.	Condensed air.	Black-bulb freely exposed.	Maximum in shade.
	NOVEMBER.					
	°	°	°	°	°	°
1861.	51·4	50·7	49·6	51·6	47·2	46·3
1862.	46·7	47·0	46·8	46·4	44·9	43·7
1863.	53·6	53·4	52·8	54·4	52·3	50·7
1864.	53·0	52·6	52·9	52·5	49·1	48·2
Means	51·2	50·9	50·5	51·2	48·4	47·2
	DECEMBER.					
1861.	47·2	45·3	45·3	48·0	44·0	44·8
1862.	49·6	49·5	49·9	49·6	48·2	48·0
1863.	49·0	49·2	48·9	49·6	48·6	48·3
1864.	43·2	43·5	43·5	43·4	41·9	43·2
Means	47·2	46·9	46·9	47·7	45·7	46·1

TABLE II.—Mean Results of the Four Years.

Month.	*In vacuo.*	In carbonic-acid gas.	In hydrogen gas.	In condensed air.	Black-bulb freely exposed.	Maximum in shade.
	°	°	°	°	°	°
January	45·8	45·1	45·1	45·8	43·8	42·1
February ...	55·2	53·9	53·8	55·2	49·3	45·1
March	69·0	67·3	66·6	66·2	59·1	48·6
April	86·5	83·5	81·5	82·3	74·6	59·3
May	93·8	90·2	88·8	89·1	79·2	64·3
June	96·7	96·2	92·8	93·8	81·3	66·1
July	102·6	102·6	98·1	98·3	87·0	71·2
August	97·6	95·0	92·5	93·9	82·0	68·4
September ...	85·2	82·3	79·6	82·1	74·0	62·6
October	69·8	67·7	66·2	68·6	62·6	57·5
November ...	51·2	50·9	50·5	51·2	48·4	47·2
December ...	47·2	46·9	46·9	47·7	45·7	46·1
Ann. means...	75·0	73·4	71·9	72·9	65·6	56·6

TABLE III.—Differences from the Readings of the Freely exposed Black Bulb in the Sun.

Month.	*In vacuo.*	In carbonic-acid gas.	In hydrogen gas.	In compressed air.
	°	°	°	°
January	2·0	1·3	1·3	2·0
February	5·9	4·6	4·5	5·9
March	9·9	8·2	7·5	7·1
April	11·9	8·9	6·9	7·7
May	14·6	11·0	9·6	9·9
June	15·4	14·9	11·5	12·5
July	15·6	15·6	11·1	11·3
August	15·6	13·0	10·5	11·9
September	11·2	8·3	5·6	8·1
October	7·2	5·1	3·6	6·0
November	2·8	2·5	2·1	2·8
December	1·5	1·2	1·2	2·0
Means	9·47	7·90	6·28	7·26

XX. *On the Composition of Ammonium Amalgam.*

By R. ROUTLEDGE, B.Sc., F.C.S.

Read October 1st, 1872.

THE substance now known as ammonium amalgam appears to have been first obtained by Seebeck* in the beginning of the year 1808, immediately after Davy had announced his brilliant discovery of the isolation of potassium and sodium by means of the voltaic battery. Seebeck prepared the amalgam by placing mercury which formed the negative pole of a battery in contact with moistened carbonate of ammonia. About the same time Berzelius and Pontin† obtained the like result with solution of ammonia.

* Annales de Chimie, lxvi. p. 191.

† Gilb. vi. p. 260, and 'Bibliothèque Britannique,' Nos. 323, 324, p. 122.

This discovery they communicated to Davy early in June 1808, declaring their conviction that ammonia, like potash and soda, must be an oxide, and that the new substance was a combination of its metallic constituent with mercury. Davy immediately commenced a series of elaborate experiments on the production and properties of the amalgam; and in an account of these experiments laid before the Royal Society in the same month* he first uses the name ammonium to indicate the supposed metallic base of ammonia. So convinced was Davy that the substance united with mercury in the amalgam was of a metallic nature, and that by combining with oxygen it constituted ammonia, that he was inclined to view nitrogen and hydrogen, if not as oxides of metals, at least as metallic gases.

Davy discovered that the ammonium amalgam was readily produced when an amalgam of potassium was made to act on moistened sal-ammoniac. He found that the electrically prepared amalgam, when introduced into a tube, rapidly evolved gas, which he describes as consisting of "about two thirds to three fourths of ammonia, and the remainder hydrogen." In another experiment, amalgam obtained by potassium was moistened with strong liquid ammonia, and when heated in a tube generated gas which was proved to consist of two thirds ammonia and one third hydrogen.

In the following year Gay-Lussac and Thénard† investigated the ammonium amalgam, and were led to regard it as a triple compound of mercury, ammonia, and hydrogen. They found, on putting some of the amalgam prepared by potassium into a tube which was filled up with mercury and then inverted in a vessel of that liquid, that the amalgam gave off, in decomposing, ammonia and hydrogen gases in the proportion of $2\frac{1}{2}$ volumes to 1. But the

* Phil. Trans. 1808, p. 355.

† Recherches Physico-Chimiques, i. p. 52.

electrically prepared substance gave off the gases in quite another proportion, the ratio in four different experiments being nearly 28 volumes of ammonia to 23 of hydrogen. These results were obtained by first drying the amalgam with bibulous paper, then introducing it into a tube containing a little mercury, closing the tube with the finger, agitating it for some minutes with the enclosed air, opening the tube after inversion in mercury, measuring the ammonia by absorbing with water, and determining eudiometrically the hydrogen mixed with the residual air. The amalgam was afterwards described by Thénard, in his 'Traité de Chimie'*, under the name of "ammoniacal hydride of mercury."

It is interesting to observe that in 1816 Ampère†, in the passage where the now universally received views on the constitution of ammoniacal compounds are first propounded, refers to the amalgam. Speaking of the difficulty of assimilating the constitution of ammoniacal to metallic salts, he remarks—"This difficulty would disappear if we admit that, just as cyanogen, although a compound body, exhibits all the properties of the simple bodies which are capable of acidifying hydrogen, so the combination of one volume of nitrogen and four volumes of hydrogen which is united to mercury in the amalgam discovered by M. Seebeck, and to chlorine in the hydrochlorate of ammonia, behaves in all the compounds which it forms like the simple metallic substances." This theory was more fully developed by Berzelius, and was soon generally received, except as regards the amalgam, concerning which various conflicting opinions were entertained. Daniell‡, for example, speaks of it as a mere mixture of mercury and gases resulting from the cohesion of the mercury and

* Vol. ii. p. 162, 3me édit.

† Annales de Chimie et de Physique, ii. p. 16 (note).

‡ Chemical Philosophy, p. 420.

the adhesion to it of the gases; and he cites the absorption of oxygen by melted silver as a similar case.

Grove*, in 1841, made a few experiments on the amalgam, and advanced the idea that it is a chemical compound of mercury and nitrogen, merely swelled up with hydrogen.

In 1864 Dr Wetherill† performed several ingenious experiments on the amalgam, but without attempting any quantitative estimate of its composition. He concludes that it is not an alloy of mercury and ammonium, and that the swelling up of the mass is due to the retention of gas bubbles by virtue of some unexplained action which he somewhat vaguely refers to catalysis.

In the 'Annalen der Chemie u. Pharmacie' for 1868‡ is a paper by Landolt, in which, after pointing out the discordance of the quantitative results obtained by Davy, and by Gay-Lussac and Thénard, he describes a method by which he attempted a new determination of the relative quantities of ammonia and hydrogen. He prepared the substance from a solution of sal-ammoniac, separated from the mercury (which formed the negative pole) by a porous cell. The amalgam, when removed from the circuit, was washed in a stream of water to get rid of the adhering solution of sal-ammoniac, which always contains free ammonia. It was then immediately plunged into dilute hydrochloric acid of known strength; and the hydrogen evolved was received in a graduated cylinder placed over it, while the ammonia was estimated by determining the amount of unneutralized acid in the liquid. Two experiments gave results corresponding respectively to 2·15 and 2·4 volumes of ammonia to 1 of hydrogen. These figures of Landolt's cannot be considered satisfactory, neither nearly agreeing with each other nor approximating to the

* Phil. Mag., United Series, xix. p. 97.

† Silliman's Amer. Journ. [2], xl. p. 160.

‡ Supp. Bd. vi. p. 346.

ratio 2 : 1 sufficiently close to justify his conclusion that they "completely confirm the results formerly obtained by Davy." Indeed Landolt points out a serious defect in his process—namely, that however rapidly the amalgam may, after washing, be transferred into the acid, the adhering water will nevertheless take up some more ammonia from the continuously decomposing substance, while the hydrogen escapes.

It must be observed that Davy himself appears to have found a difficulty in obtaining consistent results; for he does not seem to have ever entirely satisfied himself as to the proportions of the two gases. These are the words in which he sums up his observations:—"As it does not seem possible to obtain an amalgam in an uniform state, as to adhering moisture, it is not easy to say what would be the exact ratio between the hydrogen and ammonia produced, if no more water was present, than would be decomposed in oxidating the basis. But in the most refined experiments which I have been able to make, this ratio is that of one to two; and in no instance in which proper precautions are taken, is it less; but under common circumstances often more. *If* this result is taken as accurate . . . . . ", &c.*

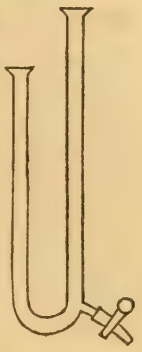
This statement of Davy's being apparently the only authority for the assertion that the decomposing amalgam gives off the gases in atomic proportions, and yet being in conflict with Gay-Lussac and Thénard's results, it appeared to me desirable to attempt to obtain more exact determinations.

I used amalgam prepared by electricity in the manner described by Landolt.

A simple mode of eliminating the disturbing effect produced by the attraction of ammonia for moisture suggested itself. A U-shaped glass tube was provided, open at both

* Bakerian Lecture, 1809.

ends, about 1·4 centimètre in diameter and having its shorter limb 40 centimètres long. At the bottom of the longer limb, just above the bend, there was an outlet tube, to which was attached a piece of caoutchouc tubing closed by a pinch-cock. Mercury was poured into the tube until it filled about two-thirds of the shorter limb, into which was then introduced the amalgam after the latter had been wiped with filtering-paper. Into the end of the limb containing the amalgam, a caoutchouc stopper, perforated with a small opening, was immediately thrust so far that its upper surface came a little below the rim of the tube. The decomposition of the amalgam was then allowed to proceed for a few minutes, during which period any moisture adhering to the amalgam or present in the tube would become completely saturated with ammonia, and the two gases would begin to escape through the perforation in the stopper in the proportions in which they are really evolved. Mercury was now poured into the open end of the longer limb until the amalgam just made its appearance at the top of the hole in the stopper, which was then closed by pushing in a piece of glass rod. The evolved gases, being thus retained in the tube, pressed up the mercury in the longer limb; and it was from time to time drawn off by the outlet-tube, to prevent undue pressure on the stopper. When the decomposition was complete, which usually occurred in about 1½ hour (but in one case more than 2½ hours were required), the mercury was brought to the same level in both limbs, and the space occupied by the gases was marked on the tube. A little mercury was then let out, so as to make the pressure on the gas somewhat less than that of the atmosphere, and the space above the stopper was filled with hydrochloric acid diluted with a little water. The glass rod was now carefully with-



drawn for an instant, so that a few drops of the acid might enter the tube. The ammonia gas present was of course immediately absorbed; and the mercury having been again brought to the same level in both limbs, the space occupied by the residual hydrogen was marked on the tube. The volumes occupied by the gases were determined by finding the quantity of water required to fill them from a burette.

The following are the results of four experiments:—

No. of experiment.	Volume of the mixed gases.	Volume of residual hydrogen.	Volume of ammonia absorbed.	Volume of ammonia found for one volume of hydrogen.
	cub. cent.	cub. cent.	cub. cent.	
1.	20·8	7·0	13·8	1·97
2.	18·2	6·2	12·0	1·93
3.	12·8	4·3	8·5	1·98
4.	13·6	4·6	9·0	1·95

I believe these figures are as nearly accordant with the atomic proportions as could be expected from the means employed, where the possible error in determining the volumes might amount to perhaps ·2 cub. centim.

In another similarly conducted experiment, in which it was sought to obtain as much gas as possible, the tube was closed too soon; and the result showed a deficiency of ammonia, but is otherwise interesting:—

Experiment 5.

Volume of mercury in the amalgam.	Volume of amalgam.	Volume of the mixed gases.	Volume of residual hydrogen.
cub. cent.	cub. cent.	cub. cent.	cub. cent.
11·8	30·5	49·0	18·0

A new observation on the amalgam has recently been made in America by Professor C. A. Seely*, who found, by subjecting it to varying pressure, that its volume changes,

* Chemical News, June 10, 1870.

apparently, in accordance with Mariotte's law. He employed simply a glass tube fitted with a plunger, and did not measure the pressures or volumes. His conclusions were that the amalgam is a mechanical or physical mixture of liquid mercury with the gases ammonia and hydrogen, and that its semifluid consistence is due to the mixture having the nature of a froth.

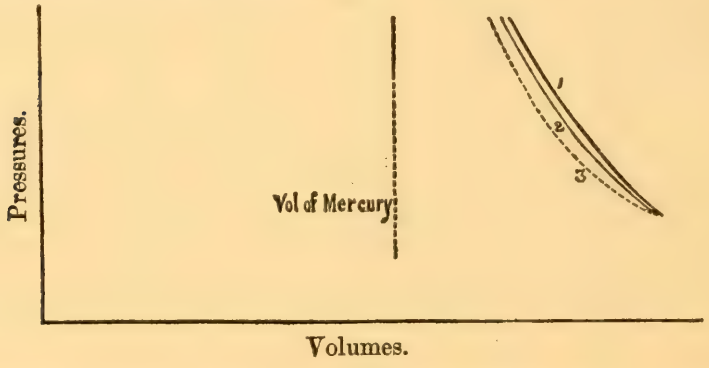
The following table contains the results I obtained on submitting the compressibility of the amalgam to the test of direct measurement.

No. of experiment.	Volume of mercury in the amalgam.	Atmospheric pressure in centimètres of mercury.	Volume of amalgam under atmospheric pressure.	The increased pressure in centimètres of mercury.	Observed volume of amalgam under increased pressure.	Calculated volume of amalgam under increased pressure.
	cub. cent.		cub. cent.		cub. cent.	cub. cent.
6.	14·5	76·2	21·0	152·4	18·0	17·9
7.	11·9	76·8	23·0	188·2	17·5	17·1
8.	11·9	76·8	22·7	200·9	17·0	16·4
9.	24·4	76·2	36·2	152·4	31·6	30·9
10.	24·4	76·2	31·6	152·4	28·0	27·4
11.	13·2	76·2	28·7	152·4	23·0	21·6
12.	13·2	76·2	22·5	152·4	18·5	17·2
13.	10·4	76·2	18·0	186·3	14·7	13·7
14.	10·4	76·2	16·0	186·3	12·8	12·8
15.	23·8	76·2	40·4	178·7	33·6	31·9
16.	23·8	76·2	42·0	176·1	33·6	32·7
17.	23·8	76·2	42·8	152·6	35·0	34·7
18.	23·8	76·2	40·4	177·4	33·3	31·9
19.	23·8	76·2	42·2	102·6	38·8	38·5
20.	23·8	76·2	42·2	153·6	34·0	34·0
21.	23·8	76·2	42·2	177·4	33·0	32·7
22.	23·8	76·2	42·0	201·5	32·2	31·6
23.	23·8	76·2	40·2	177·4	32·2	32·1
24.	23·8	76·2	40·6	201·5	31·2	30·6
25.	23·8	76·2	36·2	149·5	32·6	30·6
26.	29·2	76·2	42·0	177·4	36·8	35·4
27.	29·2	76·2	42·0	200·2	36·2	34·7
28.	29·2	76·2	40·6	173·6	36·0	34·7
29.	29·2	76·2	39·5	198·9	34·4	33·4
30.	24·6	76·2	32·0	155·9	29·7	28·4
31.	24·6	76·2	34·0	177·4	30·4	28·7

I subjected the electrically formed amalgam to pressure in a glass tube 48 centims. long and 1·3 centim. in diameter. The pressure was applied by connecting the

tube with a syringe, by which air could be forced into the apparatus; and the amount of the pressure was measured by a column of mercury in an open manometer. There was some difficulty in measuring the volume, owing to the occasional escape of bubbles of gas, which caused abrupt alterations of the level. The table also contains a column of volumes calculated on the supposition that the amalgam is a mere mixture of fluid mercury and gas, allowance being made for the pressure on the gas due to the column of mercury in the amalgam itself. The extreme case was assumed, namely that this additional pressure is represented by a column of mercury half the height of the amalgam.

Five points deduced from the mean results of experiments 15 to 24 having been laid down in relation to rectangular axes, the curve (1) which passed through them is represented in the diagram, which shows also the curve



(2) through five points representing the calculated volumes, and a line (3) representing volumes corresponding to the pressures which were applied to the top of the columns of amalgam.

The diagram and figures sufficiently show that the compressibility of the amalgam agrees nearly with the supposition of its being a mixture of gas and mercury, but that it is somewhat less compressible. This, no doubt, is owing chiefly, if not entirely, to its want of fluidity.

I think that from these experiments I am warranted in drawing the two following conclusions, viz.:—

1. In the fact of the gases being evolved in atomic proportions, we have the clearest proof that the ammonia and hydrogen are chemically combined.

2. The compressibility of the mass proves that the enlarged volume or swelling up is due mainly, if not entirely, to free gases entangled in it.

In connexion with the first of these conclusions arises the further question whether the NH_4 is combined with the mercury. That it is so combined appears in the highest degree probable from the apparently uniform diffusion of the NH_4 throughout the mass, and from the fact that such a union would be only one additional instance of the innumerable cases in which this radical plays the part of a metal. Seely says that, if the radical NH_4 be contained in the amalgam at all, it must be in the state of gas. But the figures furnished by my fifth experiment show that, if this supposed NH_4 gas had the normal molecular volume, and existed in the amalgam from the beginning, a force of two atmospheres would be required to compress it within the amalgam. The decomposition therefore is progressive, and points to the existence of a real compound of NH_4 with the mercury. We may therefore admit that such a compound is originally formed, and decomposes rapidly into mercury, ammonia, and hydrogen, while the gases becoming entangled in the mass impart to it that remarkable turgescence which is not, however, a property of the original compound (or ammonium amalgam), but merely an accidental result of its decomposition.

As to the cause of the retention of the gases, I am not prepared to offer an opinion, further than that its explanation would probably involve physical rather than chemical considerations.

I have to express my obligation to the kindness of

Dr. Roscoe for the use of the appliances of the laboratory at Owens College, where the experiments were carried out; and I am also indebted to him for valuable suggestions.

XXI. *On Arsenic from Alkali-Works.*

By H. A. Smith, F.C.S.

Read April 30th, 1872.

Some time ago I laid before this Society the results of several analyses showing the amounts of arsenic contained in the different species of pyrites used in vitriol-manufacture, and in several of the products in the manufacture of which the acid was employed. At that time I carried my analyses as far as the carbonate of soda, in which no arsenic was found. I should now like to supplement that paper by showing how the arsenic contained in the pyrites escapes in a not inconsiderable amount to the atmosphere.

When the salt used for the production of hydrochloric acid is treated with sulphuric acid containing arsenic, the arsenic present becomes converted into the trichloride ($AsCl_3$). This compound is said to be completely decomposed by contact with water, so that, after passing along with the hydrochloric acid gas through the condensing-towers, it would scarcely be expected that any traces of the arsenic originally present would be found in the escaping gas. I find, however, that this really is the case. A considerable quantity of the arsenic trichloride escapes the action of the water in the condensing-towers, and

passes, along with a very small proportion of the uncondensed hydrochloric acid gas, to the chimney.

A deposit in the flue (about 20 feet long) leading from the salt-cake furnace to the condensing-towers, the coke contained in the towers themselves, the gas in the flue leading to the chimney (through which the gases pass, having first passed through a small extra washing-tower), and the smoke escaping to the chimney were all submitted to analysis, and found to contain arsenic.

The results are gathered together in the following Tables:—

TABLE I.—Deposit in Flue leading from Salt-cake Furnace to Condensing-towers.

No. of analysis.	Arsenic trioxide. per cent.
1.	44·66
2.	42·93
3.	45·33
4.	39·98
5.	46·45
6.	44·40
7.	40·44
8.	38·97
9.	47·73
	390·89
Mean	43·43

In this Table we have the results of nine analyses of a deposit found in a flue leading from the reverberatory furnace to the condensing-towers. The total length of this flue is about 20 feet; the part from which the different samples were taken is about 15 feet from the furnace: the gas had therefore undergone cooling to a certain extent. The deposit at first sight appeared to be principally composed of common salt and a little sulphate of soda; but on examination with an ordinary hand-microscope, small octahedral crystals of arsenic trioxide

were found. On analysis this deposit gave, as an average of nine analyses, 43·43 per cent. As_2O_3. This flue had, however, been in working for some years.

Passing from the flue to the towers, the next analysis made was of the coke itself. Even when examined by the highest microscopic power at my disposal, there was no appearance of any arsenic crystals; but arsenic was very easily detected in it. For each analysis 10 lb. of the coke was used. It was digested first with distilled water, and then with perfectly pure HCl.

The results of three analyses gave 2·88 as the average percentage of arsenic present, the variation being from 2·64 per cent. to 3·18 per cent. As_2O_3. The towers had been in use for about a year.

Table II.—Coke from Condensing-towers.

No. of analysis.	Arsenic trioxide. per cent.
1.	2·64
2.	3·18
3.	2·84
	8·66
Mean	2·88

The air in the flue leading from the condensing-towers to the chimney was next examined.

For each analysis 500 cubic feet of air was employed, and twelve analyses were made.

The amount of air passing at this flue was 31,722 cubic feet per hour.

In the following Table the results have been calculated out to

Grains As_2O_3 per 1000 cubic feet,
,, per hour,
,, per day.

The numbers here are comparatively large, and the

variation considerable, owing to the fact that at one time more furnaces were in use than at another; but, as the analyses extended over a long period, the results may, I think, be taken as a very fair average.

TABLE III.—Air in Flue leading from Condensing-towers to Chimney.

(Amount of air taken for each analysis = 500 cubic feet.)
(Amount of air passing = 31,722 cubic feet.)

No. of analysis.	Arsenic trioxide per 1000 cb. ft. grain.	Arsenic trioxide per hour. grains.	Arsenic trioxide per day. grains.
1.	0·068	2·157	51·768
2.	——	——	——
3.	0·082	2·601	62·424
4.	0·072	2·284	54·816
5.	0·102	3·235	77·640
6.	0·064	2·030	48·720
7.	0·198	6·280	150·720
8.	0·248	7·867	188·808
9.	0·186	5·900	141·600
10.	0·232	7·359	176·616
11.	0·262	8·311	199·464
12.	0·382	12·117	290·808
	1·896	60·141	1443·384
Means	·158	5·012	120·282

The quantity of arsenic trioxide per 1000 cubic feet varies from 0·068 to 0·382 of a grain. This gives, as an average of twelve analyses, 0·158 grain As_2O_3 per 1000 cubic feet. Taking then the amount of air passing through the flue as 31,722 cubic feet per hour, the average amount of arsenic trioxide escaping per hour would be 5·012 grains, this becoming in twenty-four hours 120·282 grains As_2O_3. This will probably escape either as arsenious acid or as arsenic trichloride. If as the latter, it may be decomposed, on coming into contact with the atmospheric moisture, into arsenious acid and hydrochloric acid—

$$2AsCl_3 + 3H_2O = As_2O_3 + 6HCl.$$

TABLE IV.—Air taken 10 feet from bottom of Chimney.

(Amount of air taken for each analysis = 500 cubic feet.)

No. of analysis.	Arsenic trioxide per 1000 cb. ft. grain.
1.	0·046
2.	0·022
3.	0·086
4.	0·062
5.	0·112
6.	0·054
7.	0·122
8.	0·132
9.	0·144
	0·780
Mean	0·086

In Table IV. the analyses of some of the air escaping to the atmosphere from the chimney itself are given.

A small hole was made in the chimney, about 10 feet from the ground, and the passing air aspirated through. As in the former Table (Table III.), the amount of air employed for each determination was 500 cubic feet; and the number of analyses made was nine. I do not know the amount of air passing in the chimney, so have only calculated the amount of arsenic trioxide per 1000 cubic feet.

The variation in these specimens is not so great as in the former, being only from 0·022 to 0·114 of a grain per 1000 cubic feet. From nine analyses the average amount is 0·086 of a grain per 1000 cubic feet, being below the average in Table III.

The method employed for collecting the arsenic trioxide contained in the last two tables was very simple. The air was aspirated through three bottles containing respectively (1) water, (2) hydrochloric acid, (3) nitrate of silver. The gas was allowed to bubble very slowly through the solutions; the bottles containing them were capable of holding

40 ounces, and were filled about half full. It was found in most cases (I might almost, indeed, say in all) that the nitrate-of-silver solution was not required, the water and hydrochloric acid absorbing all the arsenic which was in the air drawn through.

The idea of arsenic being present in the atmosphere surrounding chemical works is by no means new. The fact of its existence in large amounts in the ore from which the greater amount of our vitriol is made leads one to suppose that it must find its way into the atmosphere at one place or another; but I believe this is the first time the comparative amounts have been brought forward. It would be very interesting to have similar analyses made on this subject in the various works where large quantities of sulphur ore are burnt, so that we could have an idea of the amount of this impurity which is being daily thrown into the atmosphere in the neighbourhood of Manchester.

I also made some attempts to determine the amount of arsenic in grass grown close to the works at which these experiments were carried on; but the result was very unsatisfactory. The grass should be got from some distance from the alkali-works, in the direction of the prevailing winds.

XXII. *On Animal Life in Water containing Free Acids.*

By H. A. Smith, F.C.S.

Read April 30th, 1872.

So much has been written lately concerning animal life, its origin, and the conditions under which it can origi-

nate and exist, that I thought it might be interesting to find out to what extent it could be present in water containing free acids, and also to notice how far free acids prevented its origination.

The animals upon which the experiments were tried were the Rotifers (*Rotifer vulgaris*). These were obtained by washing a certain amount of air in distilled water and exposing the washings to the air in a small wooden vessel, which was kept lightly covered. Five days elapsed before they came to perfection.

The method of procedure was very simple:—One litre of air in the neighbourhood of an alkali-work was washed in a glass vessel with 100 cubic centimetres of pure distilled water; this was then transferred to a small wooden vessel and allowed to stand in the laboratory, at a temperature which remained nearly constant at 60° F. Previously, however, the total amount of acidity had been obtained, which equalled 0·065 grm. per litre. This was calculated in the first place as sulphuric acid.

After having stood five days, the liquid presented a very lively appearance when viewed under the microscope, and the Rotifers seemed in a very active condition. The total acidity was now raised, by the further addition of sulphuric acid, to 0·084 grm. per litre; but there was no perceptible difference in the appearance of life. On raising the acidity still higher* (to 0·097 grm. per litre) a very decided difference was observed. The appearance of the liquid was no longer clear, but became of a light brown shade; whilst the Rotifers were apparently unwilling to move, and when any disturbance took place in the fluid their motion was very sluggish. This seemed to be a definite point in the action of the acid. Small "clots" of vegetable matter also became visible, presenting a charred

* In every case, in these experiments, on the addition of acid, the liquid was allowed to stand an entire day before further addition.

appearance. Acid was again added till the total acidity had reached 0·153 grm. per litre. The appearance now presented was similar to the former, except that life was still more languid, and after an hour all had ceased. The whole solution presented the appearance of being filled with decomposed and decaying organic matter floating about in shreds. Thinking that perhaps the length of time which had elapsed since the Rotifers had been obtained might have had some effect, a portion of the original solution which had been kept apart for the purpose, but to which no acid had been added, was examined, and the life found to be as active as at its first origination.

A number of experiments were also made in order to see what amount of free acid would prevent the origination of life. In the above case it was seen that 0·065 grm. per litre did not prevent it, that being the original acidity of the liquid. The amounts given below were then tried with the following results :—

Time allowed to stand. days.	Total acidity. grm. per litre.	Remarks.
8	0·070	Life abundant.
20	0·074	Little or no life.
26	0·080	No life.

Here, then, we see, the line of demarcation is distinctly marked, 0·004 grm. per litre making a very decided impression on, if not completely stopping, the origination of life, and 0·01 grm. per litre entirely preventing it.

The next acid tried was hydrochloric acid. The same amount of air was taken and treated in a similar manner, the acidity in this case amounting to 0·0085 grm. per litre, calculated as hydrochloric acid.

This, after being left for five days under the same conditions as formerly, was very full of life and in active condition.

The acidity was now raised to 0·01 grm. per litre, and the solution allowed to stand twenty-four hours. The appearance of the solution had not varied to the smallest degree. On further acidification till the amount reached 0·018 grm. per litre, no difference was observed between the present and the original solution. However, on the further addition of 0·001 grm. per litre (that is, on raising the acidity to 0·019 grm. per litre) a very decided difference was noticeable; all life had become extinct. The change from active life to death was almost instantaneous, and could be most distinctly seen by the microscope. The bodies of the Rotifers, generally so clearly white and transparent, became, immediately on the addition of the acid, of a dull opal-like appearance; and all movement was arrested. The solution remained clear; and the bodies of the Rotifers were seen floating in it, becoming rapidly acted upon by the acid. Even while watching, in the short space of ten minutes they became "shredded." Here, then, the line between life and death is very sharp, much more so than in the observations with sulphuric acid.

The present experiments extended over the same space of time as those in the former case—eight days from the time of washing air, or four days from time of first observation.

The same attempts were made as formerly to find the amount of acidity which would prevent life originating, the present results being:—

Time allowed to stand. days.	Total acidity. grm. per litre.	Remarks.
5	0·0085	Life abundant.
26	0·009	No life.

Thus, then, we see that the addition of 0·0005 grm. per litre of hydrochloric acid prevented the origination of life,

showing that the action of that acid on animal life is much more marked than that of sulphuric.

Sulphurous acid was next tried; and the action of this acid was very marked and rapid.

For these observations I took some Rotifers and placed them in pure water, free from acid. The sulphurous acid was then added.

On the addition of 0·002 grm. sulphurous acid per litre, the appearance of the solution completely changed. The Rotifers became more active, darting about, and making such a great disturbance in the fluid that microscopical examination became a matter of some difficulty. When this acidity was raised to 0·004 grm. per litre, they became quiet and very sluggish, a peculiar twitching action of the tail being the only indication of life.

The solution was now acidified to 0·01 grm. per litre; but by degrees all motion ceased, and about three hours after the last addition of acid all life had become extinct. The action of this acid is not so rapid as either that of hydrochloric or sulphuric acid; a smaller amount, however, is required for the destruction of animal life.

Water, acidified with 0·002 grm. sulphurous acid per litre, was treated similarly to the former solutions, to see if life could originate in water containing that amount of free sulphurous acid; but after standing twenty-one days no life was visible. I could not obtain any symptom of life in water containing this acid.

I do not consider these observations at all complete; they only show the action of these acids on one species of animal life, the Rotifers. If we took either a higher or a lower degree of life, we should probably find that our results would be in some degree different.

In this case we find that sulphuric acid has the least harmful effect on that species of animal life, hydrochloric and sulphurous acids following in order. I only lay these results before the Society thinking they may have some interest in themselves.

I have put my results into a more compact form in the following Tables :—

Table I.—Acid used, Sulphuric Acid.

Time allowed to stand.	Total acidity.	Remarks.
days.	grm. per litre.	
5	0·065	Animal life very abundant. Rotifers in very active condition.
6	0·084	No perceptible difference in the appearance of life.
7	0·097	Brownish shades now evident in the water; want of clearness in portion examined: small "clots" of vegetable matter become visible. Rotifers languid, seemingly disinclined to move.
8	0·153	Life continued for about an hour; all traces then disappeared. The water presented the appearance of being filled with decomposing and decaying organic matter, which was floating about in "shreds."

Table II.—Acid used, Hydrochloric Acid.

Time allowed to stand.	Total acidity.	Remarks.
days.	grm. per litre.	
5	0·0085	Same as in Table I.
6	0·0109	No perceptible difference in the appearance of solution.
7	0·018	No difference observable.
8	0·019	Life almost immediately extinct. Fluid still clear; bodies of Rotifers seen floating in it, but of a dull opal-like colour, and becoming rapidly acted on by the acid, seemingly becoming "shredded."

Table III.—Acid used, Sulphurous Acid.

Time allowed to stand.	Total acidity.	Remarks.
days.	grm. per litre.	
5	.....	Life very abundant.
6	0·002	Rotifers more active, causing great disturbance in liquid.
7	0·004	Life sluggish; Rotifers not inclined to move.
8	0·01	After *three* hours all life extinct; no obvious action on the bodies of the animals.

Experiments on the Amount of Free Acid contained in Water in which Animal Life can originate.

Table IV.—Acid used, Sulphuric Acid.

Time allowed to stand.	Total acidity.	Remarks.
days.	grm. per litre.	
8	0·070	Life abundant.
20	0·074	Little or no life.
26	0·070	No life.

Table V.—Acid used, Hydrochloric Acid.

Time allowed to stand.	Total acidity.	Remarks.
days.	grm. per litre.	
5	0·0085	Life abundant.
26	0·009	No life.

XXIII. *On Cometary Phenomena.*

By Professor Osborne Reynolds, M.A.

Read November 28th, 1871.

The observation of comets by powerful telescopes has shown them to be in a state of violent internal agitation —a feature which is as much their characteristic as tails or vaporous appearance; for it is not possessed by any of the planets or fixed stars. Robert Hook seems to have been the first to notice this: when he observed the great comet of 1680 (Newton's comet) through his 14-feet telescope, he saw bright streams issuing from some point near the centre of the comet's head and at first taking direction opposite to the tail and towards the sun, then gradually diverging, and finally falling back into the tail. These streams were continually changing in magnitude and direction, some of them disappearing and fresh ones appearing in their places. Their behaviour was such as to lead the philosopher to the conclusion that they were flame and smoke, or vapour excited by the action of the sun on the constituents of the body of the comet. Robert Hook again noticed this phenomenon in the comet of 1682. In 1836 Bessel observed similar appearances in Halley's comet; and, although he appears to have been in ignorance of the fact that they had been noticed before, he was led to the same conclusion as Hook as to their origin, viz. that these streams were jets of vapour caused by the action of the sun's heat on the more solid part of the comet. This hypothesis, started by Hook and afterwards by Bessel, seems to have been very generally confirmed by subsequent observation, almost all comets, large and small,

showing signs of the same action. Now although at first sight there seems nothing improbable in the supposition that the sun causes a great amount of evaporation on a comet, yet before we can admit it as altogether satisfactory it is necessary to show why the same action should not go on to the same extent on the earth and on the planets; for neither do the planets show this same appearance, nor are we aware of any action on the earth which could give rise to these appearances. I do not know that any attempts have as yet been made to explain this; but I think an explanation may be found in the difference between planets and comets in their size and the shape of their orbits, in the fact that the planets are, so to speak, large bodies moving in approximately circular paths and so remaining at about the same distance from the sun, while comets are small and move in eccentric paths, continually altering their distance from the sun. I have (in the subsequent part of this paper) endeavoured to state these reasons, and, further, to show that the difference of evaporation on a comet and on a planet is a sufficient cause for electrical phenomena on the former which do not take place on the latter.

I think that the reason why the materials of comets should at times (*i. e.* when the comets are in certain positions) evaporate under the sun's heat in a greater degree than those of planets will be rendered clear by considering the reasons why the heat of the sun does not continually evaporate the materials of the earth—always remembering:—

1st, That comets move in eccentric, and planets in nearly circular orbits;

2nd, That comets are very much smaller in mass or weight than planets.

Why, then, does not the heat of the sun evaporate the materials of the earth? The heat which the sun is

continually pouring into the earth or any of its surrounding bodies is expended in one of the three following ways:—

I. By external radiation from the body.

II. By the evaporation and liquefaction of the materials of the body.

III. By producing changes in the body such as the formation of coal and the growth of living things.

The amount of heat expended in the third way may be considered very small in any such body as the earth; for the amount of energy given out by the combustion of fuel and the work of animals must be nearly equal to that stored in the growing plants.

Therefore the heat which the earth receives from the sun during any period (a day or a year) is nearly all spent in evaporation and liquefaction or radiated away into space. Hence the quantity of heat spent in evaporation &c., is the difference between the heat received and that radiated away; and consequently it follows:—

1. If these are equal, there will be no evaporation.

2. If the heat received is greater than that radiated away, there will be evaporation &c.

3. If less, there will be condensation &c.

That is to say, if over any definite period of time the heat which the earth receives from the sun is equal to that which it radiates into space, then the amount of ice and vapour will be unchanged (unless there be some interchange between these).

If, on the other hand, the heat received is in excess of that radiated away, the vapour in the atmosphere will increase and the ice diminish, and *vice versâ*. Now the relation which the heat radiated away bears to that received will depend on two things, viz. the temperature of the earth's surface, and its distance from the sun. For both the heat received and that radiated depend in the same

way on (and, in fact, are both proportional to) the extent and nature of the earth's surface; and the quantity of heat received depends on (in addition to this) the distance of the earth from the sun (it varies inversely as the square of this distance); whereas the quantity of heat radiated away depends on the temperature of the earth's surface as well as on its extent and nature. Hence the *ratio which the heat received bears to that radiated away will decrease as the distance of the earth from the sun increases, and also as the temperature of the earth's surface increases.*

If there were nothing to melt or evaporate on the earth (or any other body whose distance from the sun is nearly constant), then the heat radiated away would eventually equal the heat received; for the temperature at the surface would continually rise until the quantity radiated away equalled that received, and there was equilibrium. This temperature I have in the remainder of this paper called the *temperature of equilibrium.* It will depend simply on the distance of a body from the sun, increasing inversely as the square of the distance. Hence in the case of planets this will be constant, whereas in the case of comets it will vary. If there is any material on the body which evaporates at a lower temperature than that of equilibrium, then there will be evaporation until the material is all gone or its conditions of boiling are altered. The temperature at which the softest material will evaporate will depend on the nature of that material and on the pressure of the atmosphere surrounding the body. Any increase in the pressure of the atmosphere will increase the temperature required to evaporate the material. If initially a body has no atmosphere, then we may assume that its materials will evaporate until the vapour forms one sufficient (if possible) to increase by its pressure the temperature of evaporation to that of equilibrium. But the possibility of this will depend on the size of the body and

the consequent attraction it has for its atmosphere; for it is clear that there must be a limit to the pressure which the atmosphere can exert on the surface of the body; and this limit must depend on the size of the body. Up to a certain point the thicker the atmosphere the greater would be the pressure on the surface; yet there must be a limit beyond which the extension of the atmosphere would produce no effect, a limit beyond which the external air would be so distant that the central body would not exert sufficient coercive force to retain it, so that all excess would go off, expanding into space. *Hence the temperature at which evaporation will continue on any body must depend on the size of the body. The smaller the body, the lower will be this temperature.*

If, then, the body is so small that the atmosphere, when at its greatest, cannot restrain evaporation until the temperature is equal to the temperature of equilibrium, then the body will go on evaporating and the vapour go on expanding into space until it is all evaporated, or, at any rate, until all the softer materials, those which evaporate at a low temperature, are gone. Thus we see that *no body whose temperature of equilibrium remains fixed—that is to say, no body which moves round the sun in a circle—no planet, in fact, can remain for ever in a condition of permanent evaporation*; for in time, no matter how long, it would lose all those materials on its surface which would evaporate at a temperature below the temperature of equilibrium. This, then, is the reason why there is not permanent evaporation going on on the earth. The temperature of equilibrium may be taken roughly at something like 50°, whereas the temperature at which the most volatile material (water) will boil is 212°. If, however, the earth were to approach the sun until its temperature of equilibrium rose to 300°, then the water would commence evaporating until either the pressure of the atmosphere

of vapour was sufficient to stop further boiling, or else until it was all gone*. Or if, on the other hand, the size of the earth were reduced so that it was no longer able to retain an atmosphere whose pressure on its surface was sufficient to prevent water boiling at 60° F., then the water would go on boiling until it was all consumed. We see, then, that the earth owes its stable condition to being so far away from the sun, and to being of such size that it can retain an atmosphere sufficient to prevent its softest material from evaporating at a temperature below that of equilibrium, and that in all bodies where this is not the case evaporation will be going on. We may now see why comets should generally be in a state of evaporation, even though they may not contain softer materials than water, and may not approach nearer the sun than the distance of the earth. The fact of their being so much smaller prevents their retaining the same pressure of atmosphere; and so their materials evaporate at a lower temperature. The eccentricity of their orbits is also essential to the explanation; for if they remained at a constant distance they must eventually lose all the material which would evaporate at that distance, and so become like the other planets. This will be the case with periodic comets, those which, in spite of the eccentricity of their orbits, return again and again; for each time they come near enough to the sun for the temperature of equilibrium to rise above that of evaporation they will lose some of their softer materials, until these are all done; and then, so far as evaporation is concerned, the comets will behave as planets.

This may appear as though it were incompatible with the existence of periodic comets. It is not so, however; it is only incompatible with the permanence of periodic

* It is true that water evaporates from its surface at a temperature much below 212°; but an atmosphere of steam would soon prevent this.

comets; and it is an explanation of the facts:—(1) that whilst there are apparently a countless number of comets which do not return, and, according to the laws of gravity, a countless number of these must have been converted by the disturbances of the planets into periodic comets, there are only a very few which are known to be periodic; (2) that the size of the periodic comets has been observed in many instances, if not in all, to decrease; (3) that there are many meteoric stones whose orbits are similar to those of many of the periodic comets, and which do not show cometic appearances,—the assumption being that numberless comets have been disturbed in their paths through space, and, instead of having been sent back in a parabolic orbit, have, owing to a second disturbance by one of our planets (generally Jupiter or Uranus) been attached to our system, and, for a time, have appeared as periodic comets such as those of Halley, but that they gradually lost their softer materials, becoming less and less, until they finally ceased to be comets, and became meteoric stones.

The rate of evaporation on such a body as a comet would obviously increase as the comet approached the sun, and diminish as it receded; but it would not depend solely on the distance of the bodies from each other; for the materials of the comet would take time to heat, and consequently, as it was approaching, part of the heat would go to warming the body of the comet, and for any position the evaporation would be less than the sun would cause if the comet were stationary. As it left the sun it would be the other way; that is, the evaporation would be more than the position warranted. Thus the greatest rate of evaporation would not be exactly at the time when the comet was nearest the sun, but some time after it had passed its perihelion. Now this lagging (as it may be called) of the sun's action on the comet is similar to, and consequently

offers an explanation of, the lagging which is observed in the display of comets. This will be seen from the following quotation from Herschel:—

"Their variations in apparent size, during the time they continue visible, are no less remarkable than those of their velocity. Sometimes they make their first appearance as faint and slow-moving objects with little or no tail; but by degrees accelerate, enlarge, and throw out from them this appendage, which increases in length and brightness till (as always happens in such cases) they approach the sun and are lost in his beams. After a time they again emerge on the other side, receding from the sun with a velocity at first rapid, but gradually decaying. It is for the most part after thus passing the sun that they shine forth in all their splendour, and that their tails acquire their greatest length and development, thus indicating plainly the action of the sun's rays as the exciting cause of that extraordinary emanation."

The direct heat of the sun would only cause evaporation on that side of the comet to which it was opposite; and consequently the stream of vapour would be emitted towards the sun, just as appears to be the case from observation; the streams of vapour would first form an atmosphere round the comet, which would increase until the extent was such that its attractive force could no longer prevent the outside being driven away by any force there might be, and so forming a tail or train—such force, for instance, as would be exerted if the sun and vapour were both charged with electricity and acted on each other by induction.

Again, as the vapour proceeded outwards from the comet it would rapidly expand; and this expansion would cause clouds to form by condensation, which would travel outwards till they were again dispelled by the sun's rays; so that on the side towards the sun it is probable

there might be one or several shells of cloud at certain distances from the central mass. These, under the action of the sun's rays, would be illuminated, and afford that striking appearance of several bands which is so often seen.

The effect of any repulsive or attractive force in the sun acting on the vapour of the comet, but not on the central mass, would cause a train or tail; but the direction of this tail would depend on the motion of the comet as well as on the intensity of the force.

It does not necessarily follow because the tail points away from the sun that the force which repels it must entirely overcome the effect of the sun's attraction; but it can be shown that even for those comets whose tails show the most curvature the force must be comparable with the sun's gravitation; and to produce the straight tails which comets sometimes possess, it would require a force of almost infinite intensity, if it acted as gravity does on ordinary matter. It has been found by Professor Norton that if the bent tail of Donati's comet were composed of ordinary matter, leaving the comet under the action of a repulsive force in the sun, this force must be between 1·5 and ·39 of the sun's attraction, the matter in the leading edge being repelled by the former, and that in the following by the latter.

If the force is electrical, its intensity will depend on the charge of electricity in the vapour; and if there are seven streams of vapour variously charged, these would each form a tail of distinct curvature; so that the comet might have several bent tails or even a fan—which are features not uncommonly observed. This, then, affords an explanation of the great variety of bent tails often seen with comets and even of those pointing towards the sun; for some of the vapour might have a charge of the opposite electricity to that in the sun, under which circumstance it would be

attracted. But, so far as I can see, the straight tails which are so often seen, and sometimes in conjunction with the curved ones, can only be explained in the manner described in my previous paper before this Society (see Memoir, Nov. 29, 1870, p. 44 of the present volume), as an electric brush or discharge through space.

As a probable cause of the electricity in the vapour of the comet, I would suggest that the evaporation taking place from the surface of the comet might cause a crust to form there, through which the subsequent vapour would have to force its way in the form of jets, which, like the jets from Armstrong's hydro-electrical machine, might issue charged with electricity, the solid being charged with electricity of the opposite kind. If this were the case, the vapour, as it formed an atmosphere round the nucleus, would discharge some of its electricity back, and so cause those portions of the atmosphere near the solid to be self-luminous; and, besides this, there might be other discharges, between the clouds or outwards into the medium of space, so as to illuminate a part of or the whole atmosphere.

On this hypothesis, if the electricity of the tail is charged with electricity of one kind, say negative, the solid head must leave the neighbourhood of the sun charged with positive electricity, which, as it gets further from the sun, and evaporation becomes feeble, will at length overpower the negative and charge the atmosphere of the comet, which would then be attracted by the sun instead of repelled; so that the tail, if it had one, would be towards the sun; or else, if by this time the comet had no atmosphere, it would carry off its charge, and (unless it lost it *en voyage*) on its next appearance it would be charged with positive electricity, and the first vapour would probably be attracted instead of repelled, and the first tail point towards the sun.

Thus a comet as it left the sun would arrive at a point where it had no tail, and afterwards would commence a tail towards the sun. This tail would not make much show on the comet's departure; for the evaporation continues to a much greater distance from the sun on the departure than that at which it commenced; but if the comet returned it would make its first appearance with a tail towards the sun, which, as the evaporation increased and the comet enlarged, would soon take the opposite direction.

There seem to have been some instances where comets have been first seen with a tail towards the sun, which they have afterwards changed; indeed this is said to be the case with Encke's comet, now visible; so that this is another instance of the remarkable way in which the actual phenomena agree with those which would result from the assumptions in the electrical hypothesis.

XXIV. *On an Electrical Corona resembling the Solar Corona.* By Professor Osborne Reynolds, M.A.

Read February 20th, 1872.

The object of this paper is to point out a very remarkable resemblance between a certain electrical phenomenon (which may have been produced before, although I am not aware that it has) and the solar corona. This resemblance seems to me to be of great importance; for the striking features of these two coronæ are not possessed by any other halos, coronæ, or glories with which bright objects are seen to be surrounded.

Until the eclipse of 1871 there was considerable doubt how far the accounts given by observers of the corona could be relied upon; but Mr. Brothers's photograph has left no doubt on the subject. In this photograph we have a lasting picture of what hitherto has only been seen by a few favoured philosophers, and by them only during a few moments of excitement.

This picture shows the beautiful radial structure of the corona, and the dark rifts which intersect it; and it also shows the disk of the moon, clear and free from light. I have not yet seen any of the photographs of the last eclipse; but I hear there are several, and that they show the radial structure and rifts even more distinctly than this one does; but whether they do or not, one photograph is positive evidence; the absence of more simply means nothing.

The features to which I refer as those which distinguish the solar corona are:—

1. Its rifts and general radiating appearance.
2. The crossing and bending of rays.
3. Its self-luminosity, shown by the spectroscopic observations of Professor Young.
4. The way in which its appearance changes and flickers.

When taken in connexion with the blackness of the moon's disk (which shows that the corona did not exist or owe its existence to matter between the moon and the plate on which the photograph was taken) these features show that we see on the card the picture of something which actually existed in the neighbourhood of the sun—that the bright rays which we see photographed were actually bright rays of light-giving matter, standing out from the sun to an enormous distance. The rifts and general irregularity of the picture show that these rays do not come out uniformly all over the sun's surface, but that they are partial and local, in some places thinly distributed, and in others absent altogether. The rays

are not all of them straight or perpendicular to the sun's surface.

Such bright rays as these cannot be the result of the sun's light or heat acting on an atmosphere or on matter circulating in the form of meteorites. If they are due to the action of the sun's light or heat at all, the matter on which these act must be distributed in the rays we see; for the sun's light and heat coming out uniformly all round would illuminate any surrounding matter, if at all, so as to show its figure.

The picture irresistibly calls up the idea of a radial emission. If it is the picture of distributed matter, that matter must exist in the form of streams leaving the sun; if it is the picture of some light-producing action, this also must exist in the form of an emission from the sun.

Such, then, are the extraordinary features of the solar corona; and, as I stated, they resemble those of an electrical corona. Any one who is familiar with the various forms of electrical disruptive discharge will recognize the general resemblance these coronal features bear to an electric brush. But to the electric phenomenon I am about to describe it is no mere general resemblance; it is an actual likeness, with every feature identical.

Before describing this phenomenon I may be allowed to state how I came to notice it. It will be remembered that in a former communication to this Society I ascribed the phenomena of comets and the corona to a certain electrical condition of the sun. Well, the peculiar appearance of Mr. Brothers's photograph induced me to try if a brass ball, brought into the condition I had ascribed to the sun, would give off a corona presenting this appearance.

The phenomenon is produced by discharging electricity from a brass ball in a partially exhausted receiver. To do this there is no second pole used, the objects which surround the outside of the glass probably answering for this

purpose. In order to produce the appearance, a certain relation is necessary between the pressure of the air and the intensity of the discharge. It is produced best when the receiver is a glass globe insulated on a glass stand, the ball being supported in its middle by a rod (coated with india-rubber, to prevent any discharge except from the ball). It is only negative electricity that is discharged into the globe*.

There is great difficulty, even when the apparatus is right, in producing the corona. Using a large coil, I just exhausted the receiver till the pressure was equal to half an inch of mercury; then there was no appearance of a corona, but one more resembling what is seen in a Geissler tube. I then let the air in gradually; and, as the pressure rose, the appearance changed at first to that of a most extraordinary mass of bright serpents twining and untwining in a knot round the ball, then to that of the naked branches of an oak tree; and as the pressure kept increasing I gradually observed amongst the branches a faint corona, which I saw at once was what I was looking for. It consisted of pencils of light, forming a faint radiating envelope round the ball, and diminishing in brightness as it receded from it. The tree gradually died out until there was nothing left but the bright radiating envelope, out of which a brighter ray would occasionally flash. The diameter of this envelope was about three or four times that of the ball. It was not steady, but flickered, so that at times it appeared to rotate on the ball. It consisted of pencils or, as they are termed, bundles of rays, between which there were dark gaps. These gaps moved round about the ball; subsequently, however, by sticking seal-

* What becomes of this electricity is not clear; when a machine is used it probably distributes itself on the inside of the glass, and induces a corresponding charge on the outside. When the coil is used it must escape back; for I have had it going for hours without any variation.

ing-wax on the ball, I rendered them definite and permanent. As the pressure of air increased, the brush became fitful, and finally ceased altogether. It was best when there was about four inches of mercury in the gauge. The phenomenon could be produced with different pressures of air by making a corresponding variation in the action of the coil; and hence I assume that there is a definite relation between the intensity of the charge in the ball and the pressure of the air surrounding it, under which the phenomena can occur.

The appearance is very faint—so faint that it is difficult to see it even when close to the ball; and I find that it takes some time before the eye can fully appreciate its beauty. It was unfortunately so faint that Mr. Brothers was unable to photograph it. The plate was exposed ten minutes; but there was not the slightest trace of any thing on it.

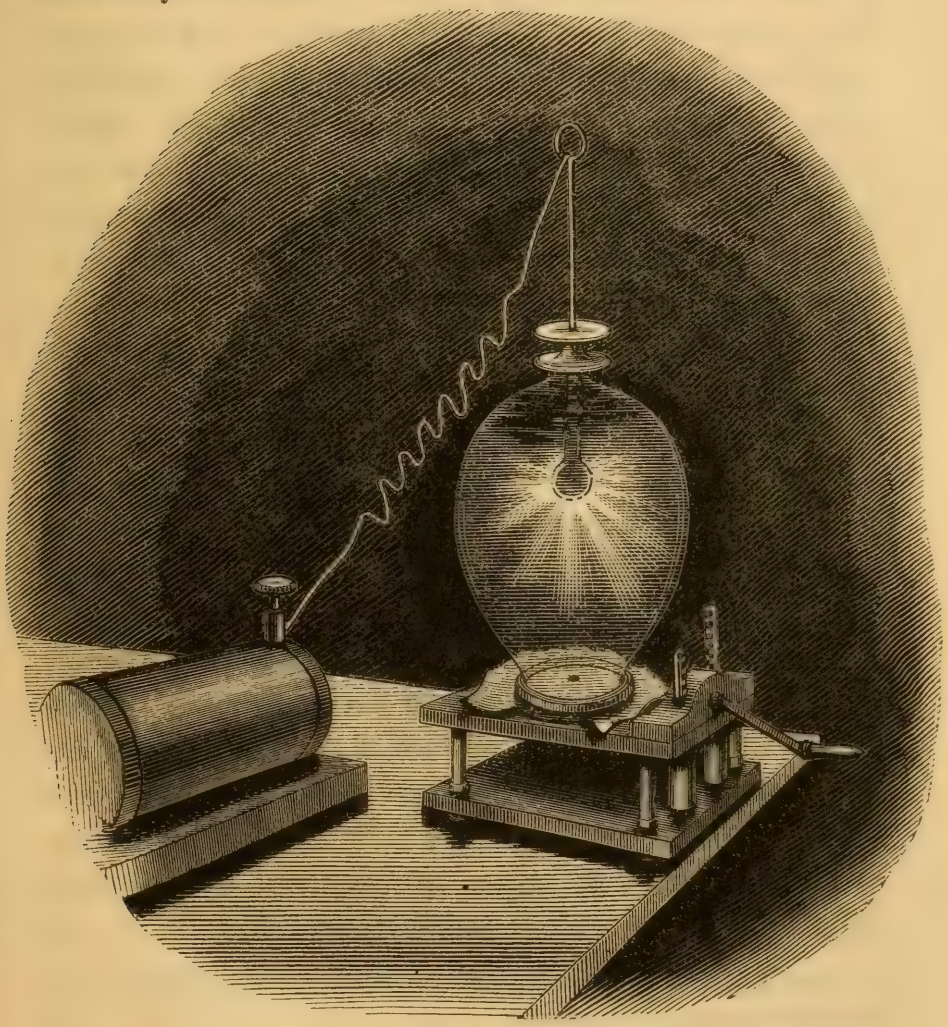
The annexed woodcut represents the apparatus employed, except that the receiver was replaced by the globe described above. The light round the ball gives a fair idea of the momentary appearance; and it is impossible to represent more, as this flickers and changes so rapidly.

This corona has the same special features as the solar corona:—

1. The rifts and general radial appearance.
2. The bending and crossing of rays.
3. The self-luminosity.
4. The changefulness and flickering.

There is one point in which it differs from the solar corona; but this is no more than must be expected. The shading off of the light in the solar corona is much more rapid than that in its electrical analogue. If, however, the pressure of the air could be made to vary so that it was denser close to the ball, even this difference could be done away with.

In this experiment, then, we have actually produced the very features which are so extraordinary in the larger phenomenon; and were there no other evidence that the solar corona is electrical, it seems to me that this resemblance constitutes a very strong proof. When two things existing at different times, or in different places, resemble each other perfectly, and resemble nothing else in the range of our knowledge, surely that is high probability that they are akin.



We may, however, expect, if the sun is electrical, to find some direct indications of its electricity. Such indications are not wanting, but are increasing every day. There are

the phenomena of the aurora and the direct effect of the sun on the electricity of the earth's atmosphere, and on its magnetism; there is, moreover, the observed connexion between the sun-spots and terrestrial meteorology, as well as the connexion between the planets and the sun-spots, shown by Mr. De la Rue and Dr. Balfour Stewart. It must be admitted that these are evident signs of some mutual influence between the sun and the planetary bodies, which is neither the result of gravity nor of heat. Almost all these signs are of an electrical character; and some are electricity itself; moreover electricity, or electric induction, is the only other known "action at a distance" besides gravity and heat. Is it not, then, probable that this influence is electrical? Are we to reject an hypothesis which explains some of these phenomena, and may explain all, simply because we do not see any cause for the electrical condition of the sun*—why the sun should be charged with negative electricity?

Should we have discovered that the sun is hot if we had waited to find out why it was so? Surely it is sufficient to say that there is no proof that it is not electrical. But we may go further than this; for, if we may compare large bodies with small, then we may show a possible reason for the sun's being electrified. When two particles of different metals approach or recede from each other, they become electrified with opposite electricities. May not the sun be approaching or leaving some other body of a different material? I do not suggest this as a probable explanation, but simply in answer to those who say that it is absurd to suppose the sun can be electrified.

* This paper was read before Section A of the British Association in 1871, when these objections were raised.

XXV. *On the Electro-Dynamic effect which the induction of Statical Electricity causes in a moving body.—This induction on the part of the Sun a probable cause of Terrestrial Magnetism.* By Professor Osborne Reynolds, M.A.

Read February 20th, 1872.

If an electrified body were placed near a moving conductor so as to induce an opposite charge in the moving body, this charge would move on the surface of the conductor so as to remain opposite the electrified body, whatever the motion might be. If we suppose the moving conductor to be an endless metal band running past a body negatively charged, the positive charge would be on the surface of the band opposite to the negative body, and here it would remain whatever might be the velocity of the band. Now the effect of the motion of this positive electricity on the conductor would be the same as that of an electric current in the opposite direction to the motion of the band.

If, instead of a band, the moving body consisted of a steel or iron top spinning near the charged body, the effect of the electricity on the top would be the same as that of a current round it in the opposite direction to that in which it was spinning.

It might be that the electricity in the inducing body would produce an opposite magnetic effect on the top; but even if this were so (and I do not think it has been experimentally shown that it would be so), its effect, owing to its distance, would be much less than that of the electricity on the very surface of the top. If we take no account of the effect of the inducing body, the current round the top would be of such strength that it would carry all the

induced electricity in the top once round every revolution. And if the top were spinning from west to east by south it would be rendered magnetic, with the positive pole uppermost—that is, the pole corresponding to the north pole in the earth or the south pole of the needle.

In order to show that such a current might be produced, a glass cylinder, twelve inches long and four across, was covered with strips of tinfoil, parallel to the axis, separated by very small intervals. These strips were about six inches long and an half inch wide, the intervals between them being the two-hundredth of an inch. In one place there was a wider interval; and from the strips adjacent to this, wires were connected by means of a commutator with the wires of a very delicate galvanometer. This cylinder was mounted so that it could be turned at the rate of twelve hundred revolutions in a minute, and brought near the conductor of an electrical machine. This apparatus, after it had been thoroughly tested, was found to give very decided results. As much as 20° deflection was obtained in the needle; and the direction of this deflection depended on the direction in which the cylinder was turned, and on the nature of the charge in the conductor. When this charge was negative, the current was in the opposite direction to that of the rotation. It may be objected that the measurement was not actually made on the cylinder. It must, however, be remembered that it was made in the circuit of metal round the cylinder, and that my object was to find the relative motion of the cylinder and the electricity. Altogether I think it may be taken as experimental proof of the fact previously stated, that, if a steel top were spinning under the inductive influence of a body charged with negative electricity, the effect would be that of a current round the top such as would render it magnetic.

The origin of terrestrial magnetism has not been the subject of so much speculation as we might have supposed

from its importance. This magnetism seems to have been regarded as part of the original nature of things, just like gravity, or the heat of the sun, as a cause from which other phenomena might result, but not as itself the result of other causes.

Yet, when we come to think of it, it has none of the characteristics of a fundamental principle. It appears intimately connected with other things; and when two sets of phenomena have a relation to each other, there is good reason for believing them to be connected, either as parent and child or else as brother and sister—the one to be derived from the other, or else both of them to spring from the same cause.

Now the direction of the earth's magnetism bears a marked relation to the earth's figure; and yet it can have had no hand in giving the earth its shape, which is fully explained as the result of other causes; therefore we must assume that the figure of the earth has something to do with its magnetism, or, what is more likely, that the rotation which causes the earth to keep its shape, also causes it to be magnetic.

If this is the case, then there must be some influence at work with which we are as yet unacquainted—some cause which, coupled with the rotation of the earth, results in magnetism. From the influence which the sun exerts on this magnetism we are at once led to associate it with this cause. Yet the cause itself cannot be the result of either the sun's heat, light, or attraction. What other influence, then, can the sun exert on the earth?

The analogy between the magnetism produced in a spinning top by the inductive action of a distant body charged with electricity, and the magnetism in the rotating earth, probably caused by the influence of the sun, which influence is not its mass or heat, seems to me to suggest what the sun's influence is. If the sun were

charged with negative electricity, it seems to me to follow, from what the experiments I have described establish, that its inductive effect on the earth would be to render it magnetic, with the poles as they actually are.

The only other way in which the sun can act to produce or influence terrestrial magnetism appears to be by its own magnetism. If the sun were a magnet, it would magnetize the earth. If this is the case, the sun's poles must be opposite to those of the earth. Now it follows that such a condition of magnetism would, or at least might, if its materials are magnetic, be caused by the rotation of the sun under the inductive action of electricity in the earth and planets, in exactly the same way as that caused in the earth by the inductive action of the sun. As the direction of rotation is the same in both bodies, and the electricities of the opposite kind, the magnetism would be of the opposite kind also. So that on this hypothesis it is probable that the sun would act by both causes.

When I first worked out this idea, I was not aware that any thing like it had been suggested before; but Mr. Baxendell, after having seen my experiments, noticed a review of a book 'On Terrestrial Magnetism,' to which he kindly called my attention. The author, without making any assumption with regard to the electrical condition of the sun, assumes it to act on the earth's electricity precisely as it would under the conditions I have described; and he then proceeds to consider, not only the general features of the earth's magnetism, but all its details (and this in a most elaborate manner)—and to show the explanation which this hypothesis offers for them, particularly for the secular variation of the direction of the needle. I am therefore able to speak of the hypothesis as affording an explanation of the numerous variations of the earth's magnetism, as well as of its general features.

XXVI. *Observations on the National Characteristics of Skulls.* By S. Messenger Bradley, F.R.C.S., Lecturer on Comparative Anatomy, Royal School of Medicine and Surgery, Manchester.

Read November 28th, 1871.

In 1856 Professor Retzius read a paper before the Scandinavian Association of Naturalists, on the present state of ethnology with reference to the form of the skull, which largely influenced the study of craniology.

The conclusions which he arrived at may be briefly summarized as follows. He arranged the various races of mankind in four classes, according to the character of their skulls.

1. Long-headed vertical-jawed people (Dolichocephali orthognathi). 2. Long-headed people with protruding lower jaws (Dolichocephali prognathi). 3. Short-headed vertical-jawed people (Brachycephali orthognathi); and 4. Short-headed people with protruding jaws (Brachycephali prognathi).

The simplicity of this classification quickly made it popular; and before long it became, and has continued to be the classification most generally accepted by English and German naturalists. My chief object is to show that we cannot any longer accept the method of classification employed by the Swedish naturalists as scientifically correct.

Amongst the European orthognathic Dolichocephali we find ranked the Germans, the Anglo-Saxons, Franks, Irish, English, Welsh, &c. I shall be able to show that in some of these nations the general type of skull is by no

means generally of a dolichocephalic character, while in all there is a large admixture of both forms.

The measurements of Professor Retzius were taken on a level with the glabella in front, and the occipital tuberosity behind—in other words, just along the line which a man's hat takes when placed upon his head. It is owing to this circumstance that I have been able to take a large number of measurements; for instead of being limited to the skulls collected in various museums (which for obvious reasons are open to the objection that they probably all belonged to people of the lower class, the significance of which circumstance we shall see later on), I have been able to measure the skulls of hundreds of living men with whose nationality and intellectual characters I was acquainted. I was enabled to obtain these measurements owing to the fact that hatters employ an instrument for taking the model of the outline of a man's skull, by which they are guided in shaping his hat. The apparatus is largely employed both in this country and abroad, and is made to adapt itself with perfect accuracy to the scalp. The closely fitting wooden pegs which form the moving part of the machine are connected with a piece of card-board by their inner extremities, which repeats the shape of the head in miniature.

I have traced the outlines of some of these forms, and wish to point out the most striking facts which they serve to illustrate. It is necessary *in limine* that I state that although the miniatures strangely exaggerate all the peculiarities of a skull, yet they may fairly be compared with each other, and, what is more to the purpose, we can in a moment obtain a correct figure of any skull by running a 2-inch gauge completely round the tracing.

It is further necessary to state that the skull will, of course, be a little (about 2 lines) less than the enlarged tracings indicate, as the scalp is included in these tracings;

but as there is not much variation in the thickness of the scalp in different heads or on opposite sides of the same head, I do not admit this to be a source of error*.

The lesson that these tracings teach is a very plain one, amounting in a word to this, that the skull-forms of (probably) every living nation range from extreme types of dolichocephalism to extreme types of brachycephalism, and constantly present examples of every intermediate form.

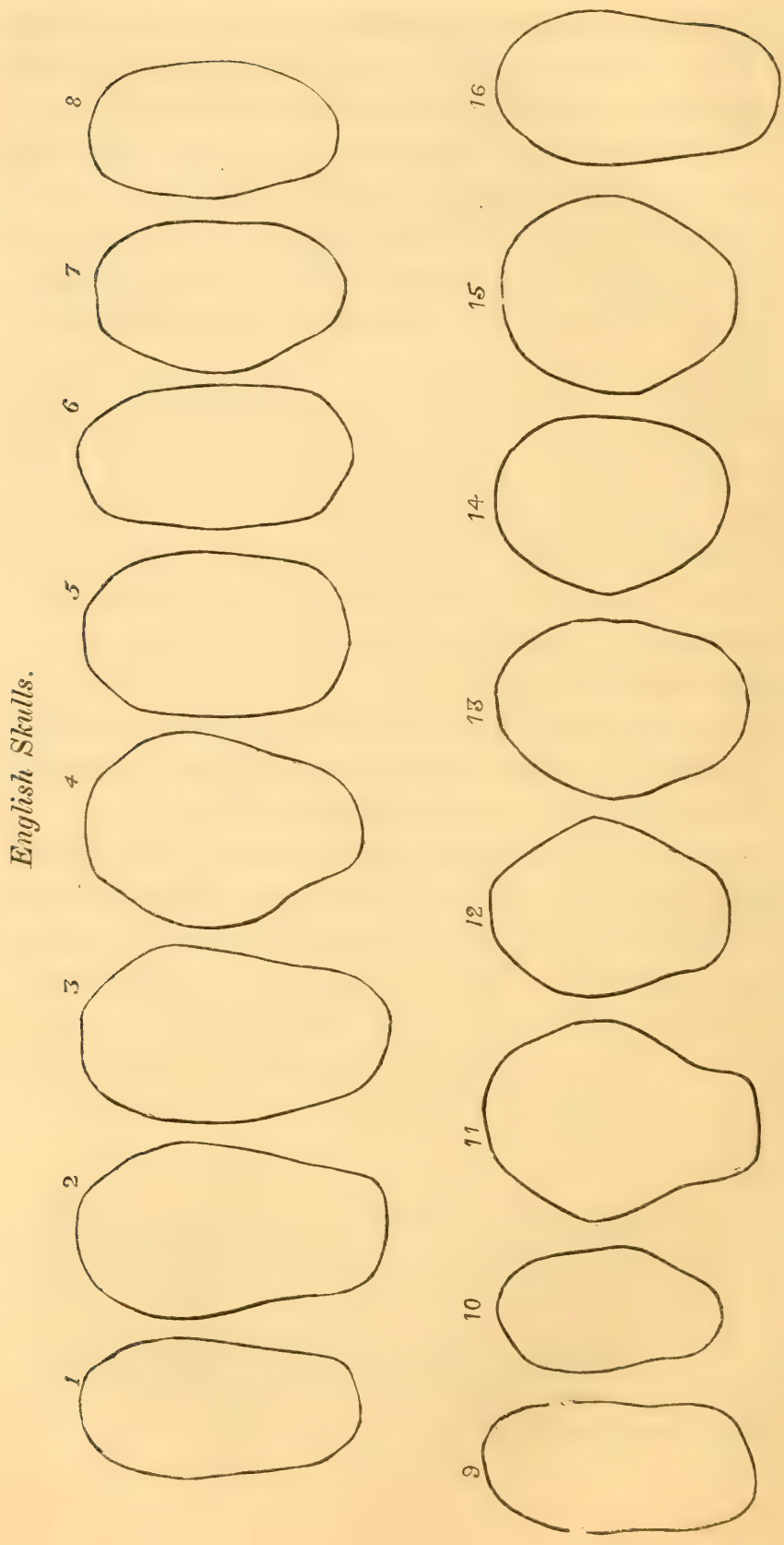
Turning to the tracings, Nos. 1–16 are English skulls. Of these, nos. 1, 2, 3, 5, 6, 7, 8, 9, 10, and 16 are dolichocephalic, 13, 14, and 15 are brachycephalic, and 4, 11, and 12 are of a median type, and may be termed mesocephalic. No. 16 is an extreme type of dolichocephalism, the skull measuring 8 inches from before backwards, and 6 inches from side to side. This gives a cephalic index of 75†.

No. 15 is a type of brachycephalism, the skull measuring $7\frac{3}{8}$ inches from before backwards and $6\frac{1}{2}$ inches from side to side, which gives a cephalic index of 88·1.

Nos. 17–28 are German skulls, which are described by Retzius as dolichocephalic. There is not a single dolichocephalic skull amongst these examples, which were taken quite haphazard. The nearest approach to dolichocephalism is afforded by No. 17, in which the skull measures $7\frac{3}{4}$ by $6\frac{3}{8}$ inches, which gives a cephalic index of 82·2. The extreme type of brachycephalism (No. 28) measures $7\frac{1}{4}$ by $6\frac{5}{8}$ inches.

* The lithographic drawings are exactly one third the size of the original tracings; so that, to obtain the real outline of any skull from them, it is necessary first of all to multiply the antero-posterior and transverse diameters by three, and then to add 2 inches to each product.

† The cephalic index is the relation which the transverse diameter of the skull bears to the antero-posterior diameter, the latter being taken at 100. When the former reaches 80, the skull is brachycephalic; when below that number, it is dolichocephalic.


English Skulls.
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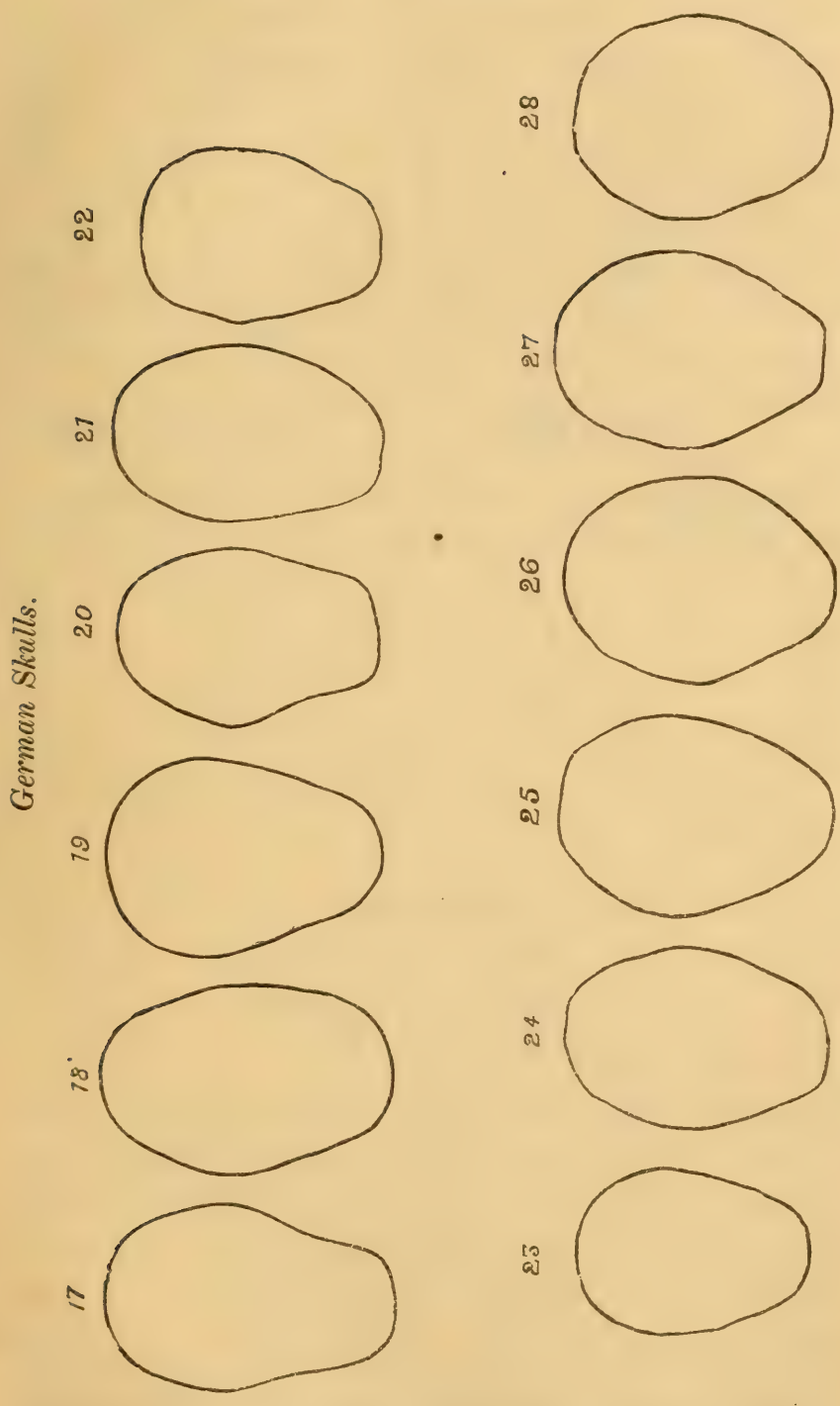
German Skulls.
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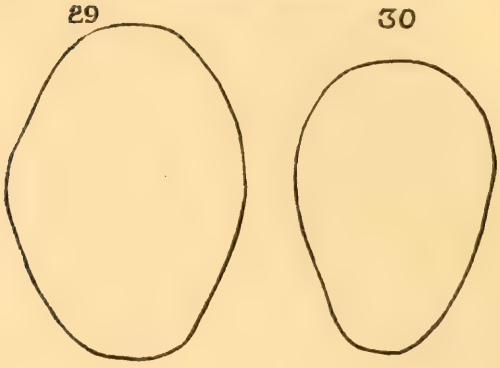
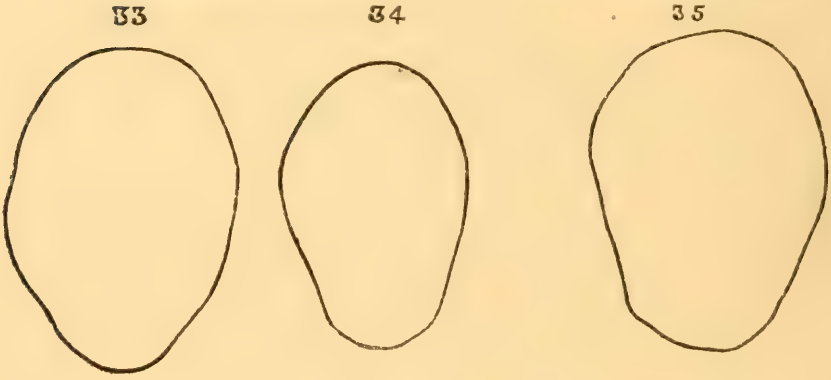
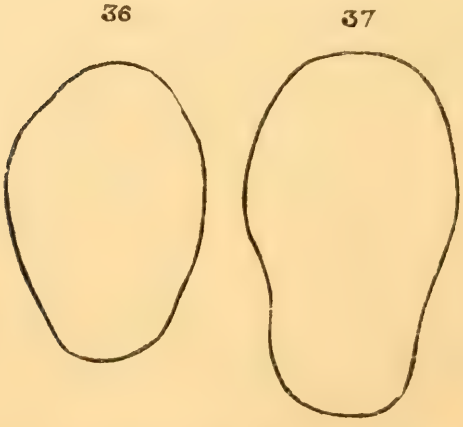
Danish Skulls.



Russian Skulls. *Spanish Skull.*



Greek Skulls.

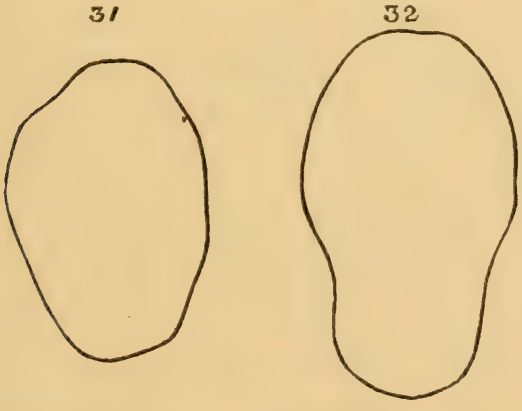


Nos. 29 and 30 are Danish skulls, both dolichocephalic. Nos. 33 and 34 are Russian skulls, one mesocephalic, and

one dolichocephalic,—the former measuring $7\frac{5}{8}$ by $6\frac{5}{8}$ inches, the latter $7\frac{3}{4}$ by $6\frac{1}{8}$ inches. Nos. 36 and 37 are Greek skulls, both brachycephalic, one (No. 36) being an extreme example. This skull measures $6\frac{3}{4}$ by $6\frac{1}{4}$ inches, which gives a cephalic index of 93, or nearly so. Carl Vogt, in his 'Vorlesungen über den Menschen, seine Stellung in der Schöpfung, und in der Geschichte der Erde,' gives many instances of brachycephalic skulls, but none so extreme as is afforded by this Greek skull. His most brachycephalic skull is that of a Klein-Russen, which measured $5\frac{1}{4}$ by $4\frac{1}{2}$ inches, the cephalic index being 86 within a very little.

Nos. 31 and 32 are Jewish skulls. The evidence afforded by the Jewish skulls is interesting. We have hitherto been

Jewish Skulls.



dealing with the skulls of peoples who freely intermarry with the peoples of other nationalities, and whose skulls in consequence may be expected to vary; but in the case of the Jew this objection no longer holds good, inasmuch as he does not intermarry, and has always kept his race free from any foreign intermixture. Do we here find a uniform dolichocephalic or brachycephalic type? By no means, but we meet with long and broad heads equally in this race with the others.

Another point which these tracings serve to illustrate is an absence of bilateral symmetry in human crania. My observations lead me, indeed, to the conclusion that, though the amount of asymmetry differs, no such thing as a perfectly symmetrical human skull exists. I am not able to say whether extreme cranial asymmetry necessarily involves a greater want of cerebral symmetry.

I now pass on to say a few words about the *facial* angle, which, Retzius states, in the Australians, Turks, Circassians, Lascars, Tartars, Negroes, &c. is always prognathic. There is no doubt that in the great majority of cases this is true; but is it sufficiently definite for a scientific classification? Is it not fair to ask what departure from a right angle suffices to constitute prognathism? Does one degree suffice, or are two necessary? When there is scarcely such a thing as a perfectly vertical jaw, these questions are important.

Again, amongst the orthognathi Retzius includes the Celtic Scotch, the Irish and Welsh; but any one who has travelled amongst these people would be able to confute the universal (or even general) accuracy of this statement, by bringing forward numerous individuals from these countries who present extreme types of prognathism. Amongst the lower class of the Irish it is the prevailing type; and there is this further interest connected with this subject, that prognathism is a feature which appears to be rapidly acquired by changed external circumstances. Before mentioning the conclusions which I draw from these facts, I may observe that no plan of measuring the dimensions of a skull which omits to note the greater or less prominence of the occipital tuberosity is likely to be of much value, as this indicates the greater or less degree to which the cerebellum is overlapped by the cerebrum. We should indeed examine the slope of the occiput as carefully and uniformly as we examine the slope and shape of the fore-

head; equally valuable evidence is afforded by the one as the other. The conclusions which I draw from these facts are briefly as follows.

It is highly probable that when the struggle for existence was less keen than at present, and the human brain was in consequence less prone to rapid growth, human skulls preserved a pretty uniform type: thus, *e. g.*, all the Neolithic skulls yet discovered are, I believe, dolichocephalic, and bear a very close resemblance to each other; further observation, moreover, shows that the skulls of prehistoric man are unusually bilaterally symmetrical. (Of the Palæolithic skulls I do not speak, as we have not a sufficient number for forming a correct opinion.) It is quite in accordance with the doctrine of evolution to suppose that different environments (such as differences in climate, soil, mode of livelihood, *e. g.* living by the chase or by agriculture) would produce certain and definite cranial changes; hence would arise national types of skull, slow in arriving at the degree of difference which exists between an Eskimo and a Negro, slow to change from that type when once acquired. After a time the modifying influence of civilization would come into operation; and we should *à priori* expect to find that the wide difference in the environments of individuals, which results from the various existing grades of society, would tend to produce a divergence in the forms of a nation's crania. We are able inductively to verify this deduction. Europe is a field where people are subjected to widely different environments; and it is precisely in Europe that we observe the hard and fast lines which Retzius made between the skulls of two nations fade away and cease to mark one type from another. A similarity of external circumstances and an absence of marriage with other nations will tend to produce but *one* type of skull; a difference in external circumstances, and inter-

marriage with other nations, will tend to produce a *varying* type. These factors are both in operation in Europe. Nations whose skulls have perhaps long ago been of a well-marked distinctive character, are exposed to the same environments and intermarry; the result is a confusion and mingling of the two forms, one cropping up here and another there.

When Retzius made his observations there is no reason to doubt that he was correct in the main; nay, it is very likely that his classification would still apply to the majority of a nation's skulls; but there is sufficient evidence in these tracings to show that so numerous are the exceptions, and so frequent the departures from one type, that a classification founded on such a system is valueless. It would be strange if it were otherwise. There is but one species of man; and however widely he may have varied in some cases from the parent stock, when their varieties become exposed to similar influences with others who have remained unchanged, he would differ from all other animals if these distinctions did not diminish and finally disappear.

One other point is of interest.

Progressive development always means greater integration and greater differentiation. The brain of the Primates becomes constantly more unsymmetrical as it becomes larger. In the Bosjesman, as in the Chimpanzee, the convolutions are comparatively simple and symmetrical. It is, to say the least of it, not improbable that this increasing cerebral asymmetry will produce some effect upon the bony cranium; and hence it is not fanciful to look upon this bilateral asymmetry, which is plainly seen in many of these tracings, as evidence, *cæteris paribus*, of a higher type than would be afforded by a perfectly symmetrical skull.

Although the commingling of long-headed and short-

headed skulls in a nation has been noticed before, sufficient weight has not been given to this circumstance in forming a cranial classification. Huxley, for example, who divides the races of mankind into two groups, (*a*) the Ulotrichi, or races with woolly hair, and (*β*) the Leiotrichi, or races with smooth hair, mentions incidentally that brachycephalic and dolichocephalic people are met with in each division; and in the four subdivisions of the Leiotrichi, viz. the Australioid, the Mongoloid, the Xanthochroi, and the Melanochroi, he states that the two forms are very freely mingled; yet he does not find any fault with the system on this account, whereas it appears to me so important us to lead us to look to the philologist, rather than to the anatomist, for a correct classification of the human race.

XXVII. *On Causes preventing Smoothness of Ground.*

By R. Angus Smith, Ph.D., F.R.S.

Read October 29th, 1872.

When in Iceland this year (1872) I spent many hours on the small Island of Effersey, in the Bay of Reikjavik, and had occasion to cross the island (only a few hundred yards) several times. It was well covered with grass of a kind not very fine; but it was unlike any surface of grass I had ever seen, although it appeared at a short distance not to be a field of unusual character, with a few risings.

It was studded over with small mounds that could be compared only to children's graves in every dimension, the spaces between being filled with grass. It was therefore very difficult to walk over the ground. Stepping from

heap to heap was a good mode, but caused a greater exertion than mere walking causes. This effort and the attempt to walk in the furrows (the least pleasant method) brought the fact before my mind very strongly. I could see no good reason for the phenomenon, although I imagined frost to be the beginning; and I also imagine that I have seen the circumstance mentioned in a periodical. However, I have asked two eminent geologists, and they have not been able to direct me to any account of such a circumstance.

Walking over the Island of Videy I saw the same state of surface in many places (not in every place); and there was a less regular form of it close to Reikjavik, and in certain places a few miles out. It was a daily wonder until, on wandering slowly on a pony towards Ellida Vatn after having gone to the right of the road to the Geysers, I saw at the side of the track small cracks in the soil forming figures about the size of the little mounds spoken of. The soil on the district now traversed had no grass, and it was closely allied to a fine gravel and had little cohesion. These figures were numerous; and the cracks were very narrow, scarcely half an inch in most cases, weaving a network of great regularity.

Then it came to my mind still more strongly that the action of frost must be at the root of it. In our own country, after thaws, we find walks with some gravel and a similar amount of cohesion as at Iceland breaking up in such a manner that some portions rise 2 or 3 inches; and there being nothing present to fill up the void caused by the separation from the rest of the surface, a crack or empty space is the consequence. With us this is done in a most irregular manner: there is no distinct form; and all the pieces are small, a few inches at most. In this bare field in Iceland the size was some 2 to 3 feet in length and less than 1 broad; and there was no rising apparent, only the separation and the crack between the

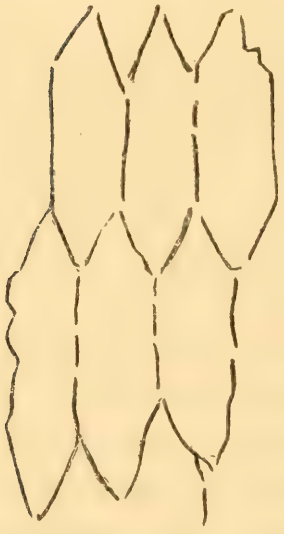
pieces. If so, we must then ask if they were quite analogous. The rising would not be expected to be seen, as I saw the place in July, and with us the ground is much levelled by rain soon after the thaw. In a grass-field in the Carse of Gowrie I noticed irregular forms of the size of those made on a gravelly walk close to it.

But if the causes are similar here and in Iceland, why is the result different? In Iceland the frost lasts longer and the hardness will be much deeper. The expansion will therefore be from a much further point of pressure, and will affect a greater surface at a time; deeper, heavier masses also will be contracting on cooling, and greater forms will take place. I think this a fair mode of reasoning.

Now we must endeavour to identify these flat forms on the gravel with the raised forms on the grass-field. The identity sets itself off very strikingly in aspect, similarity of size and form occurring in the same climate. This is a long way towards the matter already. We require now to widen the cracks and raise the forms. If grass grew on the flat forms, it would raise them by the upward pressure, but it would not grow in the crack, which the rising would widen. The crack would then become a deposit for water or a drain, and the grass would more and more seek the upper part until a natural limit was attained. What is the natural limit? As soon as the crack became so wide as to allow light and other requirements of a plant, and was so wide as to be able to take off the water rapidly by the extent of its surface, it would assert the rights of that surface, and grow grass-crops also.

This seems fair reasoning. But a difficulty occurs. The first figure, 1 foot wide, had a crack at the side about half an inch wide. The figure when raised is still 1 foot wide, and the space taking the place of the crack is equally wide. I do not see how they adjusted themselves; but it may be that in such cases the original figure was 2 feet

wide. I see no reason for supposing that those I accidentally met with in the incipient state surrounded by cracks were the normal size for all Iceland or other places.



Forms on the gravel: flat (from memory).

Forms on the grass: raised (from memory).

And now let us consider the state of a peat-bog in reference to irregularity of surface. Viewed from a height a peat-bog has frequently an appearance somewhat resembling the field described; not quite so, however. The raised parts are not regular in shape, although there is a rough similarity among them. They are also different in size, and not of the same regular dimensions as those mentioned. The furrows are wet, and are sometimes streams of water—always, one may say, to some extent draining. Have they had a similar origin, being somewhat similar? Or being somewhat similar, have they had a somewhat similar origin?

I see no reason for believing that the original form of

the raised parts was carved out by any mechanical agency on a frozen or a freezing surface. That produces the forms spoken of (if we are judging rightly) on rather dry ground. I assume that the origin is not quite the same. But the second action was considered to be that of the growing plant; and that is probably the degree required to form the peat-bog forms. One may notice that, from the earliest commencement, the plants found in them seem to grow in tufts by preference, either from a defect in the ground having few spots capable of giving nutriment or from a defect in the plants, there being few that take firm root. When once the beginning is formed, the growth is easily accounted for in the same manner.

I conclude that the results seen on the fields in Iceland are not identical in kind, and are only somewhat similar in appearance.

XXVIII. *On Anthraflavic Acid, a Yellow Colouring-matter accompanying Artificial Alizarine.* By Edward Schunck, Ph.D., F.R.S., F.C.S.

Read March 7th, 1871.

The artificial formation of alizarine is a process of very great interest to the scientific chemist as affording the first instance of the production of a natural colouring-matter by artificial means, and also to the technologist, since it is chiefly to alizarine that madder owes its valuable dyeing properties. The process itself, as described by its discoverers (Graebe and Liebermann), seems exceedingly simple. It consists essentially in the conversion of the hydrocarbon anthracene, $C_{14}H_{10}$, into alizarine, $C_{14}H_8O_4$. Nevertheless the product obtained on a large

scale (for the use of dyers and printers) by their process is very far from being pure alizarine—so much so, indeed, that some chemists are inclined to doubt its perfect identity with the natural substance. Its solution in caustic alkali, for instance, has not the fine violet colour of a solution of pure alizarine, but is more or less purple or even red. There can be no doubt, however, that the differences observed between the natural and artificial products are mainly due to impurities accompanying the latter. A few simple experiments are sufficient, indeed, to prove that artificial alizarine, as ordinarily prepared, is always accompanied by other substances, some of which are coloured while others are colourless or nearly so. My object on the present occasion is to describe one of these substances, and to point out the relation in which it stands to alizarine.

My attention was first directed to this subject in consequence of some experiments which I made to obtain alizarine from anthracene according to the directions of Graebe and Liebermann. I was surprised to find that, in spite of all the precautions taken, I always obtained besides alizarine a notable quantity of another body, also crystalline, but differing essentially from the former by its dissolving in alkalies with a yellow colour. About the same time Mr. Perkin, who, as is well known, is engaged in the manufacture of alizarine on a large scale, sent me for examination a specimen of the residue obtained by him in evaporating the mother-liquors of alizarine. This residue, which was a crystalline reddish-brown mass soluble in alkalies with a cherry-red colour, I found to contain, in addition to alizarine, a quantity of a substance apparently identical with that which I had previously obtained directly from anthracene. I afterwards found the same body, though in much smaller quantities, in commercial alizarine, both in that manufactured by the

Messrs. Perkin and in a sample from a continental firm. I therefore requested Mr. Perkin to supply me with a quantity of commercial alizarine sufficient to enable me to prepare a pure specimen of this body—a request to which he very kindly acceded.

The product supplied to me, which was a yellow, almost amorphous powder, was in the first place treated with dilute caustic soda, in which it dissolved for the most part, yielding a dark purple solution. A small quantity of pale yellow powder, consisting of impure anthraquinone, was left undissolved. An excess of acid, added to the filtered liquor, produced a bulky brownish-yellow precipitate, which was filtered off and dissolved in boiling alcohol. The alcohol, on cooling, deposited a quantity of almost pure alizarine in small mica-like scales. The mother-liquor was freed from alizarine by adding acetate of lead, which gave a bulky purple precipitate. The filtered liquid, which had a dark yellow colour, was evaporated, when it left a yellowish-brown residue, consisting for the most part of the yellow colouring-matter or acid. In order to separate the latter from the impurities accompanying it, the residue was treated first with water and then with cold alcohol, the latter of which removed a quantity of a brown resinous substance. It was then dissolved in dilute caustic soda; and the solution having been raised to the boiling-point, chloride of barium was added. The filtered liquor deposited, on cooling, a mass of small shining crystals of the barium salt of the acid. These were purified by recrystallization from boiling water and then treated with hydrochloric acid. The lemon-yellow flocks left by the acid were filtered off, washed, and dissolved in boiling alcohol. This, on cooling, deposited yellow silky needles, consisting of an acid which I have named *anthraflavic acid*, in order to indicate its source and its most obvious external property.

The chief properties of this acid are these:—When crystallized from alcohol and dried, it has the appearance of a dark lemon-yellow silky mass, which, under the microscope, is seen to consist of slender four-sided prisms. Heated on platinum foil, it gives off copious yellow fumes, and then burns with a luminous flame, without leaving any residue. When cautiously heated between large watch-glasses, it may be almost entirely volatilized, yielding a vapour which condenses in the form of a yellow sublimate. This sublimate consists of small lustrous crystalline plates, some of which, when examined under the microscope, are found to have very regular forms. The acid is only slightly soluble in boiling water, and almost insoluble in cold. It is more soluble in alcohol and ether, but insoluble in boiling benzol and sulphide of carbon. It dissolves readily in concentrated sulphuric acid even in the cold, forming a yellow solution, from which it is precipitated, by water, in yellow flocks. It is not much affected by dilute nitric acid even on boiling. In fuming nitric acid it dissolves very readily even in the cold, yielding a deep-yellow solution, which, on standing for some hours, becomes lighter in colour without evolving any gas. On now adding water, a quantity of light-yellow shining crystals is deposited, having the general properties of a so-called nitro-acid.

The salts of anthraflavic acid are compounds of well-defined character, some of them being regularly crystallized. When an alcoholic solution of the acid is mixed with an alcoholic solution of potash, it assumes a dark yellow colour, and, on standing, deposits long orange-coloured needles exhibiting considerable lustre. The sodium compound, prepared in the same manner, crystallizes in needles and resembles the potassium salt, but is lighter in colour. The ammonium salt may be obtained in dark-yellow lustrous crystals by dissolving the acid in boiling absolute alcohol and adding a slight excess of

ammonia. These crystals, however, after a short exposure to the air, lose the whole of their ammonia, leaving a yellow residue of uncombined acid. This inability to retain ammonia, even at the ordinary temperature, is a proof of the feeble nature of the acid. The potassium and sodium salts are also rather unstable compounds; for if it be attempted to recrystallize either of them from boiling water, a portion of the acid separates, the solubility of the base in water being sufficient to overcome its affinity for the acid. Anthraflavate of barium may be obtained by dissolving the acid in boiling baryta-water, or by adding chloride of barium to a solution of the substance in caustic alkali. The salt may be dissolved in boiling water without decomposition, and is deposited from the solution, on cooling, in small shining crystalline plates of a brownish-red colour, which, when examined under the microscope, are found to be very regular in form. The strontium salt is very similar, being soluble in boiling water, and crystallizing in long needles. The calcium salt, however, is insoluble in water, and is precipitated, in orange-coloured flocks, on the addition of chloride of calcium to a solution of the acid in ammonia. On adding sulphate of magnesium to an ammoniacal solution of the acid, no precipitate is produced; but, on standing some time, the magnesium salt is deposited in dark yellow crystalline plates and needles, arranged in star-shaped clusters, and possessed of much lustre. The aluminium compound, prepared in a similar manner, appears as a yellow deposit, consisting of microscopic crystals. The ammoniacal solution gives with acetate of lead a voluminous orange-coloured precipitate, with acetate of copper a light brown, and with nitrate of silver a reddish-brown precipitate.

All the compounds of the acid which are soluble in water yield yellow solutions. It is chiefly the presence of this acid in crude alizarine which affects the colour of the

alkaline solution, changing the violet, due to alizarine itself, into purple, or, when present in larger quantity, into red. From the same cause an alkaline solution of crude alizarine does not show the absorption-bands in the spectrum so distinctly as one of pure alizarine. Alkaline as well as alcoholic solutions of anthraflavic acid absorb the blue end of the spectrum very powerfully (though no bands are visible) even with very dilute solutions. A solution of the acid in concentrated sulphuric acid, however, if not too dark, shows a broad but well-defined absorption-band on the border of the blue and green of the spectrum, accompanied by a total darkening of the violet end.

If pure anthraflavic acid be dissolved in an excess of caustic potash-lye, and the solution be boiled down to dryness, a yellow residue is left, which, after being carefully heated almost to fusion, dissolves in water with a red colour. This solution contains alizarine, as it shows the absorption-bands in the spectrum peculiar to the latter—though not very clearly, on account of undecomposed anthraflavic acid still present. Pure alizarine may, however, be obtained from it by adding an excess of acid, filtering off the flocculent precipitate, dissolving the latter in alcohol and adding to the solution acetate of lead, when a purple precipitate falls, which contains the whole of the alizarine, the excess of anthraflavic acid remaining in solution. The lead precipitate, suitably treated, yields pure alizarine. It appears therefore that by the action of caustic potash anthraflavic acid is converted into alizarine. It should at the same time be stated that the conversion is never complete, probably because the action, if carried far enough to convert the whole of the acid, leads to the decomposition of the alizarine already formed.

Though anthraflavic acid gives, with bases, compounds of an intensely yellow colour, it seems to possess no dyeing properties. The freshly precipitated acid, suspended in

water, communicates not the least tinge of colour to alumina and iron mordants on calico, however long the liquid may be boiled. The effect produced in dyeing by artificial alizarine would appear therefore to be in no way due to the presence of the acid.

The analysis of the acid yielded the following results :—

I. 0·3014 grm. of the substance, dried at 100° C., gave 0·7808 grm. carbonic acid and 0·1062 water.

II. 0·3194 grm. gave 0·8288 grm. carbonic acid and 0·1140 water.

III. 0·3226 grm. gave 0·8372 grm. carbonic acid and 0·1138 water.

These numbers lead to the following composition :—

	Calculation.		Experiment.		
			I.	II.	III.
C_{15}	180	70·86	70·65	70·76	70·77
H_{10}	10	3·93	3·91	3·96	3·91
O_4	64	25·21	25·44	25·28	25·32
	254	100·00	100·00	100·00	100·00

The barium salt, prepared in the manner above described, was dried at first *in vacuo*, then at 100°, and finally at 120° C., before being analyzed.

I. 0·4980 grm. of the dry salt gave 0·8026 grm. carbonic acid and 0·1080 water.

0·4442 grm. gave 0·2528 grm. sulphate of barium.

II. 0·4844 grm. gave 0·7892 grm. carbonic acid and 0·1052 water.

0·4904 grm. gave 0·2790 grm. sulphate of barium.

The composition of the salt is therefore as follows :—

	Calculation.		Experiment.	
			I.	II.
C_{15}	180	44·22	43·95	44·43
H_{10}	10	2·45	2·40	2·41
Ba	137	33·66	33·46	33·45
O_5	80	19·67	20·19	19·71
	407	100·00	100·00	100·00

The formulæ of the acid and its barium salt are therefore respectively $C_{15}H_{10}O_4$ and $C_{15}H_8BaO_4$ + aq. It is probable that the latter, when freshly crystallized, contains some water of crystallization which is expelled at 100°, in addition to that which is retained at 120° C.; but I made no attempt to determine the amount.

The silver salt also retains water with great pertinacity, even when heated to a temperature of 140° C. The salt was obtained as a reddish-brown precipitate by adding nitrate of silver to a solution of the acid in liquid ammonia.

Its analysis led to the following results:—

I. 0·4792 grm., dried at 100° C., gave 0·6628 grm. carbonic acid and 0·0742 water.

0·4836 grm. gave 0·2882 grm. chloride of silver.

II. 0·3976 grm., dried at 100° C., gave 0·1782 grm. silver.

III. 0·4952 grm. silver salt of another preparation, dried at 140° C., gave 0·6810 grm. carbonic acid and 0·0764 water.

0·4924 grm. gave 0·2240 grm. silver.

IV. 0·5484 grm., dried at 140° C., gave 0·7616 grm. carbonic acid and 0·0810 water.

0·4714 grm. gave 0·2132 grm. silver.

In 100 parts:—

	I.	II.	III.	IV.
C	37·72	.....	37·50	37·87
H	1·72	.....	1·71	1·64
Ag	44·85	44·81	45·49	45·22
O	15·71	.....	15·30	15·27
	100·00		100·00	100·00

These numbers correspond with the formula $2C_{15}H_8Ag_2O_4$ + aq. which requires in 100 parts:—

C	37·73
H	1·88
Ag	45·28
O	15·11
	100·00

If the formula $C_{15} H_{10} O_4$ for anthraflavic acid were correct, it would stand in a very simple relation to that of alizarine, and the conversion of the acid into alizarine by the action of caustic alkalies would not be difficult to understand. If, however, the crude material which I employed was prepared from pure anthraquinone, as I am informed it was, it is not easy to see how a substance with 15 ats. C could have been obtained from it, and it may therefore be questioned whether the acid examined was perfectly pure. An experiment which I made for the purpose of removing doubt on this point, rather tends to increase it. Having heated a quantity of the acid with fifty times its weight of zinc powder, in the manner described by Graebe and Liebermann, I obtained a quantity of a brownish crystalline sublimate, amounting to about 10 per cent. of the substance employed, and consisting apparently of anthracene. It still retained, after being purified as far as possible, the yellowish tinge which, according to the chemists just named, adheres so pertinaciously to anthracene; but it did not differ in other respects from the pure substance. It melted at the same temperature as anthracene, and began to sublime before fusing; it dissolved in boiling alcohol, but more readily in benzol, and was deposited from these solutions in lustrous crystals of a very regular form; and it gave, like anthracene, with picric acid a compound crystallizing in long red needles. If, by heating with metallic zinc, anthraflavic acid, like alizarine, yields anthracene, we may infer, in accordance with the principle laid down by Graebe and Liebermann, that it also contains 14 ats. C. It is evident, therefore, that further experiments are required in order to ascertain exactly the composition and formula of the acid.

XXIX. *On Methyl-alizarine and Ethyl-alizarine.*

By Edward Schunck, Ph.D., F.R.S.

Read March 18th, 1873.

In a paper read before this Society some time ago I gave an account of a yellow colouring-matter accompanying artificial alizarine, to which I gave the name of *anthraflavic acid.*

My analyses of the acid and of its barium and silver salts led to the formula $C_{15}H_{10}O_4$ for the acid; and I was therefore inclined to view it as a body homologous with alizarine, or alizarine in which H is replaced by CH_3. I supposed it to be derived from a hydrocarbon higher in the series than anthracene, contained in the ordinary anthracene of commerce and having the formula $C_{15}H_{12}$, a body which is supposed by some chemists really to exist, and which would stand in the same relation to anthracene as toluol does to benzol. It was necessary to adopt some such hypothesis, since, as Graebe and Liebermann remark in referring to my experiments, a compound obtained from anthraquinone by the same process as that yielding alizarine cannot possibly contain 15 atoms of carbon.

The examination of anthraflavic acid was subsequently undertaken by Mr. Perkin*, whose analyses of the carefully purified substance led to the conclusion that it is isomeric with alizarine. I do not wish to dispute the accuracy of this view of its composition, since a trifling admixture of some impurity, such as anthraquinone, might easily have given rise to the excess of carbon found in my analyses—though I may state that a specimen of the substance prepared, not from commercial alizarine itself but

* Chem. Soc. Journ. vol. xxiv. p. 1109.

from some of the "by-product," from which the alizarine had already been separated before I received it from Mr. Perkin, and purified with great care, gave exactly the same composition as before.

Graebe and Liebermann* have also examined a yellow crystalline body accompanying artificial alizarine, which is converted into the latter by the action of fusing caustic potash. They are of opinion that it is identical with anthraflavic acid, there being, indeed, little or no difference in the properties of the two substances. They assign to it the formula $C_{14}H_8O_3$, and consider it monoxyanthraquinone, alizarine being dioxyanthraquinone. The results of their analyses of the substance and its barium compound differ, however, so widely from those obtained by Mr. Perkin and myself, especially as regards the number of atoms of hydrogen that may be replaced by metals, as to lead to the conclusion either that there exist more than one body having the general properties (chemical and physical) of anthraflavic acid, or that we have not all been working with pure substances.

Without pronouncing any decided opinion on this point, which can only be determined by further investigation, and without entertaining any sanguine anticipation of being able to prepare anthraflavic acid directly from alizarine, it seemed to me that it might be of some interest to ascertain the nature and properties of the methylic and ethylic compounds of alizarine obtained directly from the latter.

In order to prepare methyl-alizarine, purified artificial alizarine was treated with a mixture of iodide of methyl, caustic potash, and a little methylic alcohol in closed tubes, at a moderate temperature. After heating for some days the tubes were opened; and the excess of iodide of methyl having been evaporated, the residue was treated

* Liebig's 'Annalen,' vol. clx. p. 141.

first with hot water to remove the iodide of potassium, and then with a little cold alcohol. The alcohol (which dissolved out a brown resinous substance) having been filtered off, the residue was treated with dilute caustic potash lye, in which the alizarine not acted on dissolved with a violet colour. The liquid was filtered, and the residue, which consisted of the potassium compound of methyl-alizarine, a compound very little soluble in cold water, was washed until the percolating liquid began to be of a cherry-red colour. It was then treated with hydrochloric acid; and the orange-coloured flocks left undissolved were filtered off, washed, and dissolved in boiling alcohol. The alcohol, on cooling, deposited crystalline needles of methyl-alizarine.

Methyl-alizarine as thus prepared has the following properties:—When crystallized from boiling alcohol it appears in long yellow needles which resemble crystallized alizarine, though devoid of the semimetallic lustre peculiar to the latter. When heated it melts, and is then entirely volatilized, yielding a sublimate of yellow lustrous scales and needles. It is very little soluble in boiling water, but dissolves easily in concentrated sulphuric acid, even in the cold, giving a dark red solution. It does not dissolve perceptibly in caustic potash lye in the cold; but on boiling, a bright cherry-red solution is obtained, which on cooling deposits dark red crystalline masses. The solution shows no trace of absorption bands, but only a general obscuration of the green part of the spectrum, and in this respect differs widely from the alkaline solutions of alizarine, which exhibit such very characteristic absorption bands. The solution in concentrated sulphuric acid, however, does show an absorption band on the border of the green and blue of the spectrum, just like a solution of anthraflavic acid in the same menstruum, but far less distinctly than the latter, on account of the much greater

obscuration of the parts of the spectrum adjacent to the band. On adding alcoholic potash solution to a boiling alcoholic solution of methyl-alizarine, the potassium compound is deposited, on cooling, in dark-red needles arranged in star-shaped masses. The sodium compound, prepared in the same way, crystallizes in small light-red needles, forming a bulky mass which fills the whole liquid. In liquid ammonia and in baryta-water methyl-alizarine dissolves very little even on boiling. A watery solution of the potassium compound gives, with chloride of barium, a red flocculent precipitate, and with sulphate of magnesium a bright pink precipitate. The alcoholic solution of methyl-alizarine gives no precipitate with acetate of lead. When treated with boiling nitric acid, methyl-alizarine is dissolved and decomposed; and the solution, on evaporation, leaves a white crystalline residue, which on being heated yields a sublimate in white needles, consisting probably of phthalic anhydride. Methyl-alizarine undergoes no change when treated with strong caustic potash lye, even at the boiling-temperature. It is only when fusing hydrate of potash is employed that decomposition takes place. If the operation be carefully conducted, there will be obtained, on the addition of water to the fused mass, a violet-coloured solution, which shows the absorption bands of alizarine very distinctly. There is no doubt, therefore, that by the more energetic action of the alkali at the temperature of fusion alizarine is regenerated. Methyl-alizarine does not dye mordanted cloth when tried in the usual manner. It imparts hardly any colour to the mordants, differing in this respect from alizarine itself more than in any other.

Though methyl-alizarine differs in many points very widely from anthraflavic acid, still the two substances resemble one another in some respects. Both yield crystallized potassium and sodium compounds. Both are

converted into alizarine by the action of fusing potassic hydrate, though remaining unchanged when heated with strong alkaline lyes. The actions of the two on the spectrum when in solution are very similar. Both are incapable of dyeing mordants.

The quantity of material at my disposal barely sufficed for the determination of the composition of methyl-alizarine. The results of analysis, however, leave, I think, no doubt as to its true formula.

0·2738 grm. methyl-alizarine, dried at 140° C., gave 0·7034 grm. carbonic acid and 0·1082 water.

These numbers lead to the following composition :—

	Calculation.		Experiment.
C_{15}	180	70·86	70·06
H_{10}	10	3·93	4·39
O_4	64	25·21	25·55
	254	100·00	100·00

0·4710 grm. of the crystallized potassium compound of methyl-alizarine, prepared in the manner above described and dried *in vacuo,* lost, on being heated for several hours at 110° C., 0·0022 grm., and then gave 0·1318 grm. sulphate of potassium containing 0·0590 potassium = 12·60 per cent. The formula $C_{14}H_6(KO)(CH_3O)O_2$ requires 13·35 per cent. of potassium.

The formula of methyl-alizarine must therefore be $C_{14}H_6(HO)(CH_3O)O_2$; and hence it would appear to belong to the class of compound ethers, being formed by the replacement of one of the hydrogen atoms of a bibasic acid by methyl. It has a composition similar to that of Mr. Perkin's diacetyl-alizarine. In the latter, however, two atoms of hydrogen are replaced by acetyl. Diacetyl-alizarine seems also to be a much less stable body than methyl-alizarine.

Ethyl-alizarine was prepared in the same way as the cor-

responding methyl-compound, employing iodide of ethyl in place of iodide of methyl. The two substances have properties so similar that it is almost impossible to distinguish one from the other.

XXX. *Mean Monthly Barometric Readings at Old Trafford, Manchester, from* 1849–72.

By G. V. Vernon, F.R.A.S., F.M.S.

Read before the Physical and Mathematical Section, October 14th, 1873.

Having a tolerably complete register of barometric readings, and knowing of no published normal values for any station in or near Manchester, I have reduced and tabulated the monthly means for the period above named.

The barometer with which the observations were made was a standard, by Negretti & Zambra, No. 266, and had been compared at Greenwich.

All the observations have been corrected for capillarity and index-error to reduce them to the Greenwich standard, and then reduced to 32° F.

In the earlier part of the series I have unfortunately some months deficient; but in the latter part of the series I have been able to fill up the gaps by the use of the very careful series of observations made at Eccles by my friend Thomas Mackereth, Esq., F.R.A.S., by applying corrections determined by comparing the months in which the observations were simultaneous.

I found that the two series of observations were nearly identical, after allowing for difference of level.

The mean annual pressure for the entire series was

29·780 inches, which, corrected for an altitude of 123 feet above the mean sea-level, and assuming the mean annual temperature to be 50° F., gives us 29·912 inches as the mean yearly value reduced to sea-level.

If we take the complete period of the last eleven years, we have a mean reading of 29·838 inches, which is 0·074 inch below the value given for the entire period 1849 to 1872.

In the earlier series the month of August was generally wanting in the observations, except for a few years; but this month, having a reading generally in excess of the yearly value, would tend, if any thing, to make the annual value somewhat higher; and the result would tend to show that during recent years the pressure has been lower than during the earlier part of the series.

The variation between the highest and lowest mean yearly readings during the last eleven years is 29·828 inches (1870) and 29·624 inches (1872), or equal to 0·204 inch: this would appear greater than probability would point out, 1872 having had an exceptionally low mean reading; leaving 1872 out, we have a difference of 0·118 inch between 1870 and 1866.

Looking at the mean monthly values, we find that the maximum occurs in June and the minimum in January. The order, beginning with the minimum, is January, October, March, December, November, September, February, July and April equal, also May and August equal. If fine weather depends upon barometric pressure, the above shows that May and August should be the finest months, as a rule, and January and October the worst. May is next to the driest month April; and October bears off the palm for being the wettest, at least in this district.

The remarkable year for rain 1872 would appear to have been also remarkable for deficient atmospheric pressure, as the table annexed will show, every month except August

Barometer-Means at 32° F., *corrected for Index-error, but still requiring the Reduction to the Level of the Sea.*

(Height of barometer-cistern 123 feet above mean sea-level.)

Year.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Mean.
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
1849.	.....	.....	.....	29·549	29·784	29·885	29·779	.....	29·830	29·758	29·704	29·843	
1850.	29·876	29·752	30·069	29·580	29·740	29·890	29·856	.....	29·958	29·715	29·775	29·889	
1851.	29·540	29·883	29·573	.....	29·930	.....	29·720	.....	30·079	29·701	29·837	30·137	
1852.	29·524	29·873	30·072	30·109	29·819	29·527	29·885	.....	29·793	29·718	29·420	29·463	
1853.	29·527	29·598	29·801	29·702	29·843	29·710	29·671	.....	29·820	.....	29·923	29·857	
1854.	29·562	30·042	30·160	30·027	29·648	29·718	29·789	.....	30·004	29·682	29·689	29·704	
1855.	30·044	29·665	29·548	29·954	29·729	29·850	29·718	.....	29·994	29·475	29·896	29·709	
1856.	29·456	29·877	30·069	29·612	29·686	29·865	29·797	29·766	29·637	30·003	29·936	29·599	
1857.	29·656	29·925	29·723	29·648	29·835	29·894	29·829	29·878	29·845	29·717	30·001	30·085	
1858.	30·145	29·898	29·787	29·804	.....	29·950	29·799	29·854	29·843	29·846	29·810	29·727	
1859.	29·963	29·772	29·758	29·629	29·904	29·812	29·968	.....	29·680	29·536	29·804	29·604	
1860.	29·464	29·898	29·622	29·859	29·779	29·599	29·870	.....	29·761	29·810	29·748	29·527	
1861.	30·011	29·656	29·561	30·053	29·983	29·823	29·554	.....	29·666	29·861	29·520	29·959	
1862.	29·667	29·950	29·522	29·839	29·735	29·700	29·715	29·800	29·888	29·669	29·831	29·804	29·760
1863.	29·567	30·101	29·727	29·814	29·889	29·720	29·994	29·710	29·599	29·583	29·817	29·864	29·782
1864.	29·957	29·775	29·503	29·951	29·8?5	29·778	29·873	29·950	29·726	29·737	29·612	29·878	29·802
1865.	29·392	29·741	29·784	29·994	29·729	30·092	29·790	29·730	30·100	29·470	29·714	30·053	29·799
1866.	29·663	29·524	29·546	29·790	29·870	29·782	29·794	29·619	29·501	29·965	29·742	29·728	29·710
1867.	29·508	29·787	29·696	29·573	29·782	29·976	29·733	29·796	29·905	29·737	30·173	29·891	29·796
1868.	29·735	29·918	29·775	29·791	29·835	30·009	29·950	29·738	29·735	29·796	29·851	29·290	29·785
1869.	29·779	29 741	29·745	29·842	29·712	29·949	29·930	29·994	29·564	29·897	29·708	29·631	29·791
1870.	29·812	29·735	29·938	29·989	29·?94	29·981	29·863	29·865	29·892	29·534	29·639	29·793	29·828
1871.	29·665	29·804	29·859	29·643	29·947	29·820	29·664	29·868	29·784	29·761	29·864	29·902	29·798
1872.	29·375	29·599	29·620	29·773	29·763	29·716	29·788	29·819	29·640	29·522	29·479	29·396	29·624
Mean	29·690	29·804	29·759	29·805	29·813	29·823	29·805	29·813	29·801	29·717	29·770	29·763	

having had a pressure below the average; and August was only a few thousandths of an inch above the average:—

1872.	Difference from mean. in.
January	−0·315
February	−0·205
March	−0·139
April	−0·032
May	−0·050
June	−0·107
July	−0·017
August	+0·006
September	−0·161
October	−0·195
November	−0·291
December	−0·367

Comparison of these departures below the average with the separate monthly values of the rainfall for 1872 does not show that the greatest rainfall occurred in those months in which the pressure was the most below the average; but the almost constant depression throughout the year certainly does so: it is probable that, if a longer and more complete register could be examined, 1872 had a lower mean pressure than any year over a very long period, just as it had a greater rainfall than any year for a very long period, say eighty years.

XXXI. *Notes on fossil Lithothamnia (so-called Nulliporæ).*

By Arthur William Waters, F.G.S.

Read January 27th, 1874.

The organisms on the table before us have been assigned very various places in the organic or inorganic kingdoms

at different times. The recent ones have perhaps most often been placed among the corals, while the fossil forms have passed most frequently as concretionary.

Lamarck, Deshayes, Milne-Edwards, D'Orbigny, Bronn, and others placed the Nulliporæ among the Anthozoa, either as a separate genus or as a division of the Milleporæ. Ehrenberg placed them under his Pocillopora, but, while doubtful what is their true nature, does not think they are plants. Blainville and Link consider them to be concretions; and such the fossil forms have usually been considered to be by geologists. Murchison and Sedgwick, speaking of the limestone of Wildon, Styria, from which place a few of those on the table were collected, say, "many of the masses have a mottled appearance, resulting from a number of spheroidal and cylindrical concretions formed of concentric layers of white carbonate of lime, probably produced, in the first instance, by organic bodies, the traces of which are now lost."

In 1837 Philippi*, by microscopic examination of some recent forms, proved their vegetable nature, establishing nine species of what he called *Lithothamnium*, and placed them near to *Corallina*. Kützing, later, entirely confirmed their vegetable nature, calling them Spongites.

In 1866 Rosanoff† published a classical paper on the Melobesiaceæ, which he divides into three sections—*Melobesia*, *Lithophyllum*, and *Lithothamnium*. In this paper he gives full descriptions as to their mode of growth and their reproduction.

Philippi, Kützing, and Rosanoff confined their attention to the recent forms. Unger, of Vienna, in 1858‡ proved, by microscopical examination, the similarity of organisms of the Leithakalk.

* Wiegmann's 'Archiv für Naturgeschichte,' iii. Jahrg. 1837.

† Mém. de la Soc. Imp. des Sc. Nat. de Cherbourg, t. xii.

‡ Denk. der Ak. der Wissensch. math.-naturw. Cl. Bd. xiv. (Wien).

Since then Gümbel has published, in 1871, an excellent monograph* on the fossil forms, under the title "Die sogenannten Nulliporen," in which he establishes fifteen species. This paper has been largely used in preparing the present notes.

This would seem to be the best place to mention their importance in geological formations. As most present will know, they attain their greatest development in the Leithakalk, a Miocene formation, which is principally, in some cases almost entirely, composed of this Alga. Unger says he has never seen any which contains less than two thirds. Thus formations of vast extent and many hundreds of feet thick, perhaps in some places thousands, are largely composed of this limestone seaweed. But it is in no way confined to the Leithakalk, being also very abundant in the Eocene, especially the upper divisions; the so-called "Granitmarmor," or Bavarian marble, a Nummulitic formation, is very largely composed of this concretionary-looking body. From what I have seen, I should judge that more than half is thus made up.

This is a very good building-stone, largely used in Bavaria. In all the large buildings in Munich (as the Glyptothek, Pinakothek), and many of the buildings at Vienna, this marble is extensively used. In North Italy it abounds in the Eocene formations which are so largely developed in the Veronese and Vicentin. Some of the pieces before you will show what a large proportion some of the rocks there contain. In many places the formation is some hundred feet, much more than half composed of the *Lithothamnium*.

It occurs abundantly in Hungary and Switzerland. The so-called Pisolithic limestone of Paris is, according to Gümbel, about four fifths stone Algæ, also Monte Mario, Astrup, the Pliocene of Castel Arquato; and, in fact, it

* Abhand. d. k. bay. Ak. der Wissensch. xi. part 1 (München, 1871).

seems to be found in most of the Tertiaries on the Continent. It is further found in the Chalk at Maestricht and in the Jurassic sponge-beds at Schwabenbergs (in the Frankischen Alb).

A calcareous seaweed (the *Corallina reussiana*) has, according to Schimper, been found in the Silurian beds in Bohemia, the oldest known at present.

There are a few fine fossil specimens on the table from the Murray River, Australia, kindly lent to me by Mr. William Johnson. The few fossils from the same place, from a cursory examination, seem to correspond with the European younger Eocene or Miocene; but I cannot say exactly their position, though it is undoubtedly middle or later Tertiary. It is very difficult to say much about its distribution, since, from the extreme simplicity of appearance, it is but seldom noticed by geologists or travellers; but doubtless, when geologists have paid more attention to the question, we shall find it has a very wide distribution, and has formed rocks of equal importance in other parts of the world.

Taking the Eastern Alps, with which I have a fair acquaintance, viz. the Bavarian, Austrian, and North-Italian Alps, in which, as you know, the Tertiary deposits attain extraordinary importance, I do not think it is too much to say that one sixth of the whole Tertiary rocks of this district are composed of *Lithothamnium*. I do not give this as an exact calculation in any way, but merely to give an idea of its importance.

The rocks formed by corals will only bear a very small proportion to those thus formed.

It would be very strange if this fossil, which is so abundant on the Continent, should not occur frequently in England; but, as far as I am at present aware, it has not been mentioned from any locality in Great Britain.

The recent Melobesiaceæ seem to have a very general

distribution, principally in the North temperate and Arctic zones. They grow at but moderate depths, and are apparently fairly common on the west coast of the British Isles, especially on the coast of Ireland.

Lithothamnium belongs to the family Melobesiaceæ of the order Florideæ or Rhodospermeæ. A large number of this order are lime-secreting; but these (*Lithothamnium*) differ very materially from the Corallinæ, since in the former the carbonate of lime is deposited in and between the cell-structure, while with the Corallinæ the plant is filiform, and the lime is deposited round this, entirely incrusting it. I think the structural difference of the Melobesiaceæ from the Corallinæ and other limestone-secreting seaweeds is sufficient to separate them into two entirely distinct classes, since the similarity of stony appearance is merely morphological, the deposition of lime being so different.

The reproductive organs are produced in conceptacula, called by Rosanoff cystocarpi. The spores, tetraspores, and antheridia are known of many recent species.

One of the microscopical sections, of which a drawing is before you, shows some of the conceptacles of this fossil cut through. There can, I think, be no doubt of their true nature. There is also a body which I take to be a tetraspore; but I feel it to be far from certain: as the tetraspores would contain but very little carbonate of lime, they could be but imperfectly fossilized. The conceptacula are, in recent species, according to Rosanoff, free from carbonate of lime.

Gümbel, after saying how frequently only fragmentary pieces are to be had, and not fine bunches, and how the stems and branches of a species are subject to variation as well as the external characteristics and the superficial nature, although some are easily distinguishable by the character of branching and superficial form, says, "but by

far the most important and numerous species can scarcely be distinguished by any other method than by the form and relative size of the cells, which can only be made out by transparent sections." We must be very careful how we accept this; for in a piece I prepared, from Torbole, on the Lake of Garda, a drawing of which is shown, we find the same plant producing distinctly three sizes of cells—at first the cells being large; then at right angles to these cells grow out others much smaller, the same process being repeated a second time. The three sets of cells are in the proportion 5 : 3 : 2. In any branch we take we shall find the centre cells, which are arranged along the long axis, much larger than those at the sides, which turn outwards.

According to Rosanoff, the form of the young is always orbicular. The germinal cell divides into two; these again divide; then the cells grow concentrically round these, usually soon losing the orbicularity, and growing principally in one direction by occasional radial division of the cells. The cells, when cut through in a branch at right angles to the direction of growth, are circular.

Gümbel gives the average of various analyses of the *Lithothamnium nodosum* from Vienna and Monte Mario :—

Lime	47·14
Magnesia	2·66
Alumina, iron, and oxide of manganese	2·55
Phosphoric acid	0·06
Carbonic acid	40·06
Insoluble in acid	4·96
Water and loss	2·57
	100·00

You will notice, beside the large amount of carbonate of lime, the large proportion of magnesia, representing 5½ per cent. of carbonate of magnesia.

Gümbel points out the important bearing the large amount of magnesia which plants and animals can take

up from the water and convert into carbonate, may have upon the question of the formation of dolomitic rocks.

The *Lithothamnium* takes very various forms—in some formations abounding as small concretionary-looking lumps, in others in branched form. Some recent ones at the British Museum are tubular, others filiform; some anastomose; very frequently others grow quite straight. One Melobesiacea in the British Museum is composed of fine filiform tubes, about six or eight together, these bundles then frequently anastomosing. The fossil ones are very frequently much altered in colour. One rock on the Lake of Garda might almost be described as a black rock with white spots, an infiltration having taken place from the outside, turning all but the central portion a dark colour, so that each piece, when broken through, shows the centre white and the rest dark. I bring this before your notice to show the great care that is required when judging from lithological characteristics, as no one would, at first sight, and often not without microscopical examination, consider such a pure white rock, and one almost black, to be composed of the same material.

From the mode of its growth, it will easily be understood that solid masses can be formed of such stony seaweeds, since they grow incrusting stones, corals, and other *Lithothamnia*, in this way filling up all the interstices. In some instances, in rock-sections, I have found the chambers of Foraminifera thus filled up; probably they were partially broken before the commencement of the vegetable growth.

As I before said, the difficulties are at present very great in studying this organism, on account of the want of material and observation; but it is very important every geologist should look carefully for it, since it should be a very material help in regard to the climate, and the con-

dition of the coasts and currents, besides being of great stratigraphical assistance.

In conclusion, the object of this paper is to draw attention to the great masses of these bodies, and the importance of always noticing their occurrence in geological formations. Nor is it of less importance to note carefully the growth of recent ones; for only through a knowledge of the present can we interpret the past.

XXXII. *On the Graphical Representation of the Movements of the Chest-wall in Respiration.* By Arthur Ransome, M.D., M.A.

Read January 13th, 1874.

In a communication to the Royal Society * I have given a description of a three-plane stethometer, an instrument for measuring simultaneously the extent of movement of points on the chest-wall in three directions at right angles to one another (namely, forward, upward, and outward) during one act of breathing. Some of the results obtained with this instrument were also given; and in subsequent papers, to the Royal Society and to the Medico-Chirurgical Society †, I have given both the practical applications of the instrument, and the physiological deductions which may be drawn from these measurements.

In these researches, amongst other matters, certain indications were observed which showed that the course described by the end of a rib, in respiratory action, was very far from being regular, and very different from the curve

* Proc. Roy. Soc. vol. xxi. p. 11.

† Medico-Chirurgical Transactions, vol. lvi. 1873.

which it would produce if its movements were governed simply by the mechanical conditions of the costo-vertebral joints, and by the length and obliquity of the chord-line drawn from its anterior extremity to its head.

In consequence of these indications, it seemed desirable that exact graphical tracings of the course described by the ends of the ribs should be obtained, in order that their peculiarities in different cases might be studied.

The chief difficulty in the way of obtaining these tracings was found in the fact that the course of the movement lay in three planes, instead of in two only.

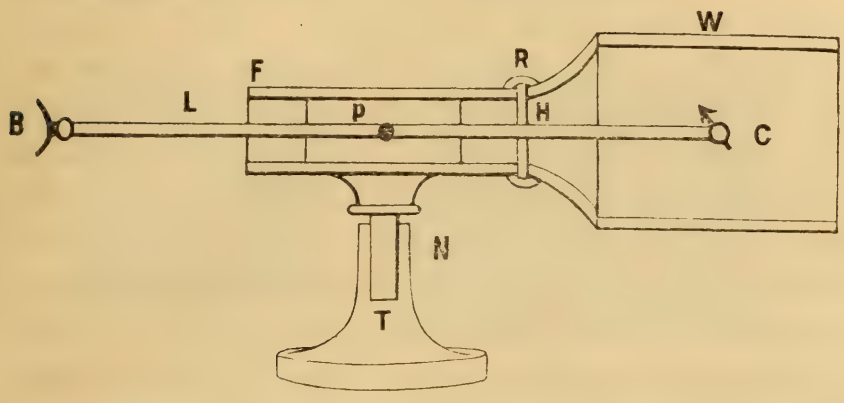
Observations with the three-plane stethometer, however, had shown that the degree of outward or lateral motion possessed by the ends of the ribs was very small, and that the measurements of this movement were of much less importance than those of the other two, which carried with them interesting practical indications.

It seemed better, therefore, to sacrifice the registration of this, the lateral motion, for the sake of bringing the upward and forward movements into the same plane. It was easy to do this by resting the whole apparatus, both the pencil and the ground upon which the course was described, upon a swivel joint, which permitted free rotation with any lateral movement of the chest-wall; and in experiments upon the ends of the ribs this motion is so small, that it produces no perceptible effect upon the movements in the upward and forward direction, and any error that it introduces may safely be neglected.

The stethograph, then, which I have devised consists of a lever (L), 6 inches long, having a pad (B) at one end, connected with it by a ball-and-socket joint, and so shaped as to fit upon the rib, and at the other a finely-pointed pencil (C).

This lever is suspended at its central point by a pivot (P), which permits of free movement up and down, but in

no other direction, and which firmly fixes the lever to a sliding-plate working backwards and forwards in a slot in the frame (F).



A writing-frame (W) is attached to the instrument by a hinge (H); and a spring (R) keeps it continually pressed against the pencil at C. The whole mechanism is then allowed to rotate upon the pivot (N) in the stand (T).

It will be readily seen that these arrangements permit the motion of the pencil (C) backwards and forwards, and upwards and downwards, and that these movements will closely correspond to those of the pad (B), with the difference that the upward movement of the pad will produce a downward thrust of the pencil, and *vice versâ*; and thus, when the pad is placed upon the end of a rib, all its movements, with the exception of that directly outward, will be transferred in reverse to the paper or smoked glass contained in the writing-frame; and by having the paper ruled in squares, each one tenth of an inch, the extent of movement in the forward or upward direction can be read off at once.

In using the instrument it is fixed to a table by a clamp, or by a sufficiently heavy weight attached to its foot, and the back of the subject to be examined is firmly supported by a pad placed opposite the costo-vertebral articulation of the ribs whose movements are to be traced. In this

manner any error due to the altered curvature of the spine in respiration is avoided.

It will be observed that the medium for receiving the writing is fixed, so far as its relation to the pen is concerned—that it is not a rotating cylinder, as in Dr. Sanderson's stetho-cardiograph, nor a travelling carrier, as in Dr. Marey's sphygmograph. These devices are needed when a record of the relative force and frequency of the motions is required; but, on the other hand, the introduction of this extraneous movement prevents the accurate delineation of the actual track pursued, in the forward and upward directions, by any point on the chest-wall. By means of the instrument now described, this tracing may be obtained without difficulty.

The following curves are selected in illustration of the results which have been obtained with the stethograph; and, for the sake of affording a simple basis for comparison, they are in the present paper limited, for the most part, to the movements of the ends of the third pair of ribs.

I. *Movements in Health.*

In observations made with the three-plane stethometer, it had been remarked that the forward motion of the rib was most equable throughout the whole act of respiration, and that in men the upward movement takes place chiefly at the latter portion of the respiratory act, but that in women and children this movement keeps pace with the forward push.

Figs. 1 and 2, from a healthy adult male, and figs. 12, 13, 14, and 15, from a woman and from a youth, entirely bear out this statement. In the tracing No. 2, taken from the right third rib of an adult male, it will be found that the ordinates of the curve in the first half of its course are 0·9 inch of forward to 0·3 inch of upward motion, and in

the latter half they are 0·4 inch forward to 0·75 inch upward. In the female and the youth the curves are much more equable in the two directions of motion.

It may also be noticed in these healthy curves that the lines of the tracing enclose an irregular but yet a perceptible space—in other words, that the anterior end of the rib takes a different course in its ascent from that of its expiratory descent.

In most cases the uppermost line is that of inspiration; and when it reaches its highest point it descends somewhat more abruptly than it rose, being afterwards drawn inwards more horizontally. In other instances, however, this track is reversed, and the descent of the rib is more gradual than its ascent (see figs. 9 and 10); and in some the two tracks will interlace with one another once or twice; or they may occasionally, but very rarely in healthy breathing, follow the same line.

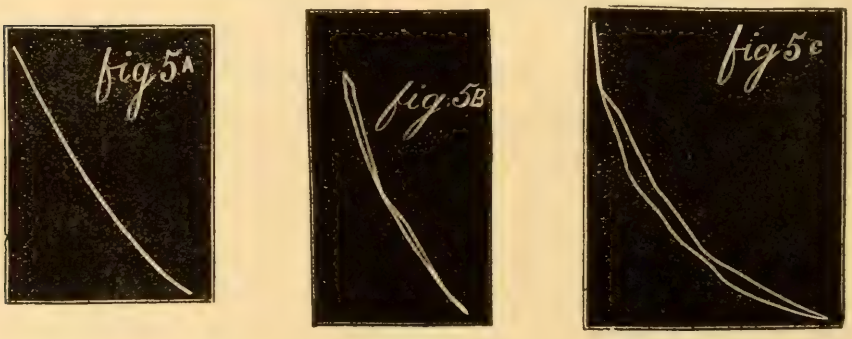
These variations are doubtless produced by the varying degree to which the will interferes with the action. The expiratory act in most persons is at first automatic, and mainly due to the elasticity of the parts; but when a forced effort of expiration has to be made, the voluntary muscles of respiration are used to a greater or less extent according to the will of the subject.

Now it is important to observe that the actual shape of this respiratory curve is very different from what it would

be on the usually received hypothesis, that the ribs are simply rigid bars moving upon the support of their costo-vertebral joints.

It is not difficult to construct the curve which ought to be described by the end of the rib on this hypothesis. The average chord-length of the third rib is known; and the expiratory and inspiratory angles made by the plane of the rib with the vertical can easily be ascertained. Fig. 5 A gives the curve produced with a radius of 6 inches and an initial angle of 60°, conditions resembling those of the chest from which figs. 1 and 2 were taken.

Again, another, more direct means of ascertaining the curve formed by the end of the rib in its rise and fall may be taken; and fig. 5 B represents the actual motion of the



fifth rib in a dead male subject, in whom the fourth and fifth intercostal spaces had been divided, and the costo-sternal attachment released, so as to permit of free movement at its vertebral articulation. The rib was then simply raised and depressed, whilst the button of the stethograph rested upon it.

In fig. 5 *c* some inward pressure was made upon it during its descent, and gradually removed during its ascent.

Now the curve constructed by the simple rise and fall

of the rib considered as a rigid body must necessarily possess the following attributes :—

1. It must approximate to a segment of a circle.
2. It will be the same in its ascent and descent.
3. Its shape for the same rib will always be nearly the same.
4. Its curve will depend (*a*) upon the angle formed by the plane of the rib-circuit with the perpendicular, and (*b*) upon the length of the radius, the chord-length of the rib employed.

It is hardly needful to point out that the first and second of these characteristics are not found in the actual curves described by healthy ribs; and, in order to test the third and fourth, an attempt was made to ascertain what influence could be exerted upon the movement by the constraining power of the will. In figs. 3 and 4 the result is given in

one case (3) of an effort to raise the rib with but little forward motion, and (4) to push its end forward without raising it.

Although the tracing is irregular from the constraint put upon the ordinary motion, it is evident that in No. 3 we have a closer approximation to the simple radial movement; and in No. 4 the effort has succeeded in producing 1·2 inch of forward push with only 0·6 inch of rise.

These peculiarities of the motions of the rib in health

can only be accounted for by an alteration in the chord-length of the rib, due to the action of some constrictor-power possessed by certain of the respiratory muscles.

The tracings of the spasmodic and partly involuntary actions of coughing and sneezing are also interesting, not only from their bearing upon the mechanism by which these acts are accomplished, but also because they afford a strong corroboration of the conclusions drawn from ordinary breathing.

In fig. 6 it may be observed that, after the first rapid

inspiration, which traces an oblique but almost rectilinear line on the paper, there is a momentary depression, as if the rib were commencing to assist in expiration; but this is suddenly stopped by the closure of the glottis, and there is a slight outward bulging of the rib to the extent of 0·05 in. This appearance may probably be explained by the continued action of the diaphragm, which compresses the air in the chest, and so, to some extent, forces the ribs outwards for the moment. Immediately that the air is released from the windpipe, however, there is at once a downward fall of the rib for a space of about 0·2 in., with a barely perceptible incline inwards; there is then a sudden change in the direction of its course, and it is drawn inwards for about 0·4 in. almost horizontally, the fall in this space amounting only to about 0·15 in.

In the first part of the expiratory effort only is there any similarity to the hypothetical curve given in fig. 5.

The second half is entirely unlike any thing that could be radially produced by the rib, and can, in my opinion, only be explained by the bending of the rib, which, as I have shown before, usually occurs in forced expiratory efforts.

The second act of coughing, which is portrayed in the figure, is almost exactly like the first, except that, as might have been anticipated, there is no slight drop preceding the second sudden closure of the glottis.

In the second figure belonging to the same case (fig. 7),

there are three acts of coughing in the same fit; and it is noticeable that, although the general method by which they are accomplished is the same, the second and third acts are apparently each preceded by a very short inspiratory effort. In all, however, we notice the outward bulge upon the closure of the glottis; and it is very interesting to note that the extent of this forward push became greater the lower the rib descended, as if the rib yielded to the pressure of the air in the chest more easily in the position of partial expiration than in that of full inspiration.

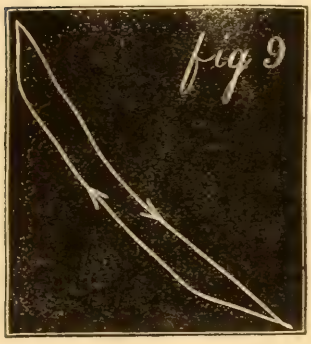
In the act of sneezing, produced by snuff-taking, shown in fig. 8, the course taken by the rib is remarkably like its

track in coughing. There is first the almost rectilinear

track of quick inspiration, and afterwards the downward drop of the rib, followed immediately by a very strong indrawing of its end. The differences observable are the absence of any stoppage at the commencement of expiration, and the much more complete indrawing in the second half of the expiratory act. The first portion is strikingly similar in its course to the hypothetical curve, its ordinates being about 0·5 in. of downward drop for 0·15 of indrawing; those of the curve given in fig. 5 are 0·8 downwards and 0·2 inwards.

In the second part of its expiratory track the end of the rib only drops about 0·15 in. to 0·8 in. of indrawing, and for six tenths of an inch its course is almost horizontal. I would again repeat that such a movement seems to me impossible without an inbending of the rib.

Figs. 9, 10, and 11 are introduced to show the variations



occasionally produced by individual peculiarities:—first, in

the course of the inspiratory and expiratory tracings; and, second, in the form of the cough-movements, the closure of the glottis taking place after some degree of expiration had been accomplished.

Figs. 12 and 13 display the movements of the third ribs

in a healthy female, æt. 29 years. The pliability of the bones is well shown both by the large extent of forward movement in proportion to the upward rise, and by the variations in the curves of inspiration and expiration.

In the boy, æt. 11, from whom the tracings given in figs. 14 and 15 are taken, the same elasticity is apparent,

and the curves also demonstrate the great freedom of movement possessed by the thoracic walls in childhood.

Figs. 16 and 17 mark the differences in the healthy tracing produced by advancing years. There is an approach to the form of curve traced by the unyielding rib;

and the upward and downward track are more nearly alike.

II. *Movements in Disease.*

A few tracings (figs. 18–28) are given to show the effect of disease upon the form of the respiratory curves :—

Figs. 18, 19, and 20 are from a female, æt. 21, suffering

from chronic phthisis on both sides of the chest. Duration 9 years. Fig. 18 is from the least-diseased side, fig. 19 from the third rib over a large contracting vomica. Fig. 20 represents a cough.

Figs. 21 and 22 are from a male, æt. 25, also a case of

phthisis, but of an acute character, the tracings being taken during an intermission of the disease.

Fig. 23 is from the right third rib, and fig. 24 from the left third rib, in a male subject of chronic phthisis, in whom the left side was chiefly affected. His age was 35, the duration of the disease 4 years.

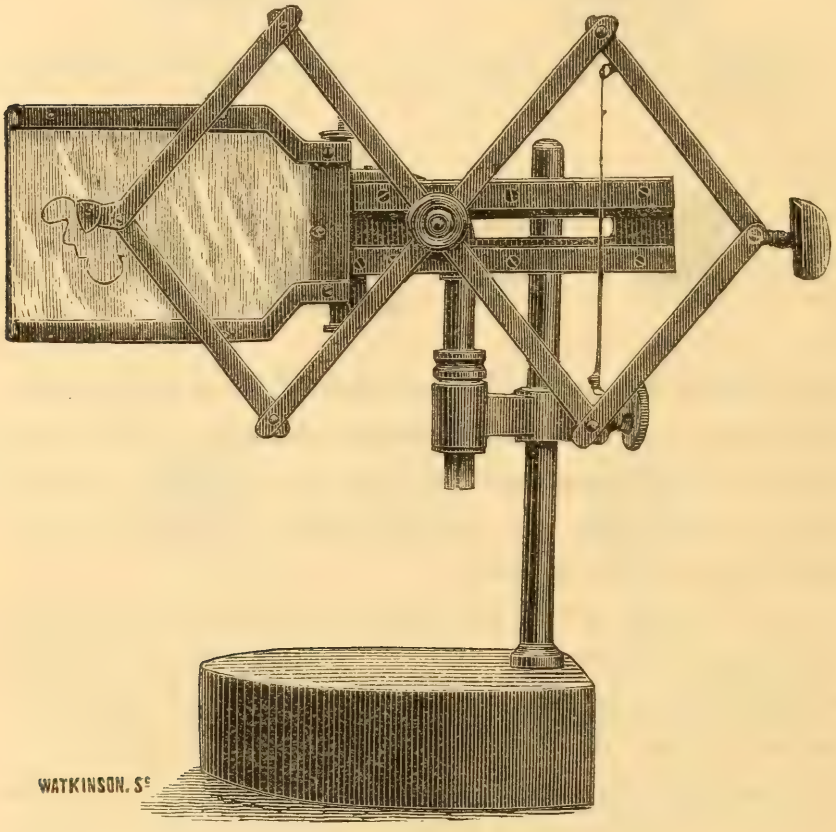
The four tracings, figs. 25–28, are taken from a female,

æt. 26, who had acute pleurisy with effusion nine months before, and in whom there was considerable contraction of the left side in consequence. The first two are from the third rib, the last two from the fifth. Figs. 26 and 28 are from the diseased side.

In all these cases the comparative feebleness of the respiratory acts are to be noticed, and the want of elasticity of the chest is evidenced by the tendency to similarity in the upward and downward track of the end of the rib. In the case of acute phthisis there is also a degree of tremulousness in the original tracing; and the case of pleurisy displays the effect of the subsequent adhesions in the very small extent of the forward push on the affected side. It is interesting to notice that the phthisical cough portrayed in fig. 20 is similar in its form to that of the healthy chest fig. 6, although so much smaller and more feeble.

Note.—Since this paper was written, a modification of the instrument has been kindly suggested by Mr. Gibbon, Assistant in the Physiological Laboratory at the Owens College. In order to obviate any risk of error from the

influence of the upward motion of the lever upon the record of the forward push, he proposed to substitute a pentagraphic motion for the sliding-stage (P); and the apparatus, as it is now constructed, presents the appearance shown in the figure.



A large number of observations have been made with this new form of stethograph; but the tracings do not materially differ from those given in the paper, nor do they lead me to alter the descriptions which I have given of the several curves. The alteration of the instrument is an important one, however, and renders the results theoretically more trustworthy.

XXXIII. *An Improved Method for preparing Marsh-gas.*

By C. Schorlemmer, F.R.S.

Read November 18th, 1873.

Every one who ever had to prepare soda-lime knows that the preparation of this substance is a troublesome as well as a laborious process. Chemists will therefore hail with pleasure a paper "On the Determination of Nitrogen," by S. W. Johnson (Liebig's 'Ann.' vol. clxix. p. 69). He has found that, in using the method of Varrentrapp and Will, soda-lime may be replaced by an intimate mixture of about equal weights of anhydrous sodium carbonate and dry slaked lime. It occurred to me that such a mixture might also be employed instead of soda-lime in the preparation of marsh-gas; and I found that by heating an intimate mixture of anhydrous sodium acetate with more than twice its weight of lime and sodium carbonate, a very regular and quiet evolution of marsh-gas took place. The gas thus obtained always contains some acetone, which is easily removed by shaking it with water or, better still, with a solution of acid sodium sulphite.

XXXIV. *The Chemical Constitution of Bleaching-powder.*

By C. Schorlemmer, F.R.S.

Read December 2nd, 1873.

In his classical research "On the Compounds of Chlorine with Bases"* Gay-Lussac has shown that the bleaching

* Compt. Rend. vol. xiv. p. 927.

compounds formed by this reaction are not direct combinations of chlorine and a base, as Berthollet believed, but that a hypochlorite and a chloride are produced simultaneously, according to the equation

$$2\,KOH + Cl_2 = KOCl + KCl + H_2O.$$

When to the compounds thus formed a small quantity of a mineral acid is added, hypochlorous acid is set free; whilst by adding the acid in excess, only chlorine is obtained, because in the latter case the hydrochloric acid acts on the hypochlorous acid in the following way:—

$$ClH + ClOH = Cl_2 + H_2O.$$

As a ready method for preparing a dilute solution of hypochlorous acid, Gay-Lussac recommends to distil a solution of bleaching-powder with a quantity of dilute nitric acid which is just sufficient to liberate the hypochlorous acid.

According to Gay-Lussac's view, bleaching-powder is a mixture of calcium chloride and calcium hypochlorite; and the same view is held by most chemists; Prof. Odling, however, has pointed out that, calcium being a dyad metal, the constitution of bleaching-powder was probably $Ca\begin{cases} Cl \\ OCl' \end{cases}$ or it was, at the same time, a hypochlorite and a chloride. Of course both views explain equally well the formation of hypochlorous acid by Gay-Lussac's method; and I read, therefore, with great surprise a paper by Goepner (Dingler's Polytechn. Journ. vol. ccix. p. 204), in which he states that bleaching-powder is nothing but a simple combination of lime and chlorine, which, by acids, is again resolved into its constituents without the least trace of hypochlorous acid being formed. He says that although the preparation of hypochlorous acid by this method is described in all handbooks as if this experiment had been

made hundreds of times, this is a mistake, and the reason why this error has maintained itself so long in chemical literature is, that hitherto no reaction was known by which free chlorine and hypochlorous acid could be readily distinguished. But such a reaction has now been found by Wolters, who has shown that when chlorine-water is shaken with an excess of mercury, only mercurous chloride is formed, while with aqueous hypochlorous acid it yields a brown crystalline oxychloride of mercury, which is readily soluble in hydrochloric acid, and thus offers a ready means of the qualitative as well as quantitative determination of hypochlorous acid in the presence of free chlorine.

In employing this reaction for detecting hypochlorous acid in the liquid which was obtained by distilling bleaching-powder with a small quantity of hydrochloric or sulphuric acid, Goepner could not find a trace of hypochlorous acid, but only free chlorine.

I have already mentioned that he says the preparation of hypochlorous acid by this method is described in the books as if this experiment had been repeated hundreds of times. Now this experiment has been repeated many hundred times in our laboratory only. Professor Roscoe shows it every year in his lectures; and all our students, in the course of their practical work, perform it, and find that the perfectly colourless distillate is a much more powerful bleaching-agent than freshly prepared chlorine-water. This is quite sufficient to show that the liquid contains hypochlorous acid. But why did Goepner fail in detecting it? Perhaps it was the fault of the analytical method. To decide these questions I prepared hypochlorous acid by distilling solutions of bleaching-powder with dilute nitric and sulphuric acid, and shook the colourless distillates with mercury. In every case the brown oxychloride was formed in quantity, and possessed all the properties which Wolters has assigned to it; while by

shaking chlorine-water with mercury, only calomel was formed. From a careful perusal of Goepner's paper I was unable to find the cause of his failure.

Another argument against the existence of a hypochlorite in bleaching-powder is, according to Goepner, the following. The chlorine which is used in the manufacture of bleaching-powder always contains free hydrochloric acid, and thus, in bleaching-powder, more calcium chloride will always exist than would correspond to Gay-Lussac's formula. Now, when bleaching-powder is exhausted successively with small quantities of water, the excess of calcium chloride is always found in the first solutions, whilst those following contain calcium and chlorine in the proportions corresponding to the empirical formula $CaOCl_2$. This fact, however, only proves that bleaching-powder is not a mixture of calcium chloride and hypochlorite, but that the bleaching compound contained in it has the constitution which Prof. Odling has assigned to it.

Professor Williamson has shown that an aqueous solution of hypochlorous acid may also be obtained by suspending finely divided calcium carbonate in water and passing chlorine into the liquid until the carbonate is dissolved, and then distilling the solution. In this reaction the compound Ca(OCl)Cl is probably also first formed and acted on by an excess of chlorine in the following way :—

$$Ca\begin{cases} Cl \\ OCl \end{cases} + Cl_2 = Ca\begin{cases} Cl \\ Cl \end{cases} + Cl_2O.$$

XXXV. *Some Remarks on Dalton's First Table of Atomic Weights.* By Prof. Henry E. Roscoe, Ph.D., F.R.S.

Read November 17th, 1874.

As the Society is aware, the First Table, containing the relative weights of the ultimate particles of gaseous and other bodies, was published, as the 8th and last paragraph to a paper by Dalton, "On the Absorption of Gases by Water and other Liquids," read before this Society on October 21, 1803, but not printed until the year 1805. There appears reason to believe that these numbers were obtained by Dalton after the date at which the paper was read, and that the paragraph in question was inserted at the time the paper was printed. The remarkable words with which he introduces this great principle give us but little clue to the methods which he employed for the determination of these first chemical constants, whilst in no subsequent publication, as in none of the papers which have come to light since his death, do we find any detailed explanation of how these actual numbers were arrived at. He says* "I am nearly persuaded that the circumstance [viz. that of the different solubilities of gases in water] depends upon the weight and number of the ultimate particles of the several gases, those whose particles are lightest and single being less absorbable, and the others more, according as they increase in weight and complexity. An enquiry into the relative weights of the ultimate particles of bodies is a subject, as far as I know, entirely new. I have been lately prosecuting this inquiry with remarkable success. The principle cannot be entered upon in this paper; but I shall just subjoin the results, as far as they appear to be ascertained by my experiments."

* Manch. Mem. vol. i. 2nd ser. p. 286.

Here follows the Table of the relative weights of the atoms:—

"*Table of the relative weights of the ultimate particles of gaseous and other bodies.*

Hydrogen	1
Azot	4·2
Carbone	4·3
Ammonia	5·2
Oxygen	5·5
Water	6·5
Phosphorus	7·2
Phosphuretted hydrogen	8·2
Nitrous gas	9·3
Ether	9·6
Gaseous oxide of carbon	9·8
Nitrous oxide	13·7
Sulphur	14·4
Nitric acid	15·2
Sulphuretted hydrogen	15·4
Carbonic acid	15·3
Alcohol	15·2
Sulphureous acid	19·9
Sulphuric acid	25·4
Carburetted hydrogen, from stagnant water	6·3
Olefiant gas	5·3."

In the second part of his 'New System of Chemical Philosophy,' published in 1810, Dalton points out, under the description of each substance, the experimental evidence upon which its composition is based, and explains, in some cases, how he arrived at the relative weights of the ultimate particles in question. Between the years 1805 and 1810, however, considerable changes had been made by Dalton in the numbers, the Table found in the first part of the new system being not only much more extended, but in many cases the numbers differing altogether from those given in the first Table published in 1805. It is therefore, unfortunately, to a considerable extent now a matter of conjecture how Dalton arrived at the first set of numbers;

all we know is, that it was mainly by the consideration of the composition of certain simple gaseous compounds of the elements that he arrived at his conclusions; and, in order that we may form some idea of the data he employed, we must make use of the knowledge which chemists at that time (1803–5) possessed concerning the composition of the more simple compound gases.

As I can find no record of any explanation of these early numbers, I venture to bring the following attempt to trace their origin before the Society to whom we owe their first publication.

The first point to ascertain, if possible, is, how Dalton arrived at the relation between the atomic weights of hydrogen and oxygen given in the Table as 1 to 5·5 (but altered to 7 in 1808). The composition of water by weight had been ascertained by the experiments of Cavendish and Lavoisier to be represented by the numbers 15 of hydrogen to 85 of oxygen; and this result was generally accepted by chemists at the time, amongst others doubtless by Dalton. That in those early days Dalton had actually repeated or confirmed these experiments appears improbable. At any rate he formed the opinion that water was what he called a binary compound, *i. e.* that it is made up of one atom of oxygen and one atom of hydrogen combined together. Hence, if he took the numbers 85 to 15 as giving the composition of water, the relation of H = 1 to O would be 1 to 5·6, or nearly that which he adopted. It does not appear possible to explain why Dalton adopted 5·5 instead of 5·6 for oxygen; it may, perhaps, have been a mistake or a misprint, as there are two evident mistakes in the Table, viz. 13·7 for nitrous oxide instead of 13·9, and 9·3 for nitrous gas instead of 9·7.

Let us next endeavour to ascertain how he obtained the number 4·3 for carbone (altered to 5 in 1808 and to 5·4 later on). Lavoisier, in the autumn of 1783, had ascer-

tained the composition of carbonic acid gas by heating a given weight of carbon with oxide of lead, and he came to the conclusion that this gas contained 28 parts by weight of carbon to 72 parts by weight of oxygen. Now Dalton not only was acquainted with the properties and composition of carbonic acid, but he was aware that Cruikshank had shown, in 1800, that the only other known compound of carbon and oxygen, carbonic oxide gas, yields its own bulk of carbonic acid when mixed with oxygen and burnt; and also that Desormes* analyzed both these gases, finding carbonic oxide to contain 44 of carbon to 56 of oxygen, whilst carbonic acid contained, to 44 of carbon, 112 of oxygen, being just double of that in the carbonic oxide. Dalton adds, "this most striking circumstance seems to have wholly escaped their notice." Hence Dalton assumed that one atom of carbon is united, in the case of carbonic oxide, with one atom of oxygen, whilst carbonic acid possessed the more complicated composition, and contains two atoms of oxygen to one of carbon. Now, if carbonic acid contains carbon and oxygen in the proportion of 28 to 72, carbonic oxide must contain half as much oxygen, viz. 28 of carbon to 36 of oxygen, and assuming that the atomic weight of oxygen is 5·5, that of carbon must be $\frac{28 \times 5{\cdot}5}{36} = 4{\cdot}3$. Having thus arrived at the number 4·3 as the first atomic weight of carbon, it is easy to see why Dalton gave 6·3 as the atomic weight of carburetted hydrogen from stagnant water, and 5·3 as that of olefiant gas. The one represents one atom of carbon to 2 of hydrogen; the other, one of carbon to one of hydrogen; or, olefiant gas contains to equal quantities of carbon only half as much hydrogen as marsh-gas. This conclusion doubtless expressed the results of Dalton's own experiments upon these two gases, which were made, as we

* Ann. de Chimie, t. 39. p. 38.

know from himself, in the year 1804. He proved that neither of these gases contained any thing besides carbon and hydrogen, and ascertained, by exploding with oxygen in a Volta's eudiometer, that if we reckon the carbon in each the same, then carburetted hydrogen contains exactly twice as much hydrogen as olefiant gas does, and that "just half of the oxygen expended on its combustion was applied to the hydrogen and the other half to the charcoal. This leading fact afforded a clue to its constitution." Whereas in the case of olefiant gas two parts of oxygen are spent upon the charcoal and one part upon the hydrogen.

The atomic weight of nitrogen (azote = 4·2) was doubtless obtained from the consideration of the composition of ammonia, whose atomic weight is given in the Table at 5·2. Ammonia was discovered in 1774 by Priestley; but the composition was ascertained by Berthollet in 1775 by splitting it into its constituent elements by means of electricity, when he came to the conclusion that it contained 193 parts by weight of hydrogen to 807 parts by weight of nitrogen. Dalton assumed that this substance is a compound of one atom of hydrogen with one of nitrogen, and hence he obtained for the atomic weight of azote $\frac{807 \times 1}{193} = 4{\cdot}2$; and $4{\cdot}2 + 1 = 5{\cdot}2$ as the atomic weight of ammonia. It is also probable that Dalton made use of the composition of the oxides of nitrogen for the purpose of obtaining the atomic weight of nitrogen. If we take the numbers obtained partly by Davy and partly by himself, as given on page 318 of the 'New System,' as representing the composition of the three lowest oxides, it appears that the mean value for nitrogen is 4·3 when oxygen is taken as 5·5. In all probability the number in this Table (4·2) was obtained from an experiment of Dalton's made at an earlier date.

It is not possible to ascertain the exact grounds upon which Dalton gave the number 7·2 for phosphorus; its juxtaposition, however, in the Table, to phosphuretted hydrogen shows that it was probably an analysis or a density-determination of this gas which led him to the atomic weight 7·2, under the supposition that this gas (like ammonia) consisted of one atom of each of its components. In the second Table, published in 1808, Dalton gives the number 9 as that of the relative weight of the phosphorus atom; and we are able to trace the origin of this latter number, although that of 7·2 is lost to us. On p. 460, part ii. of his 'New System,' Dalton states that he found 100 cubic inches of phosphuretted hydrogen to weigh 26 grains, the same bulk of hydrogen weighing 2·5 grains. Hence $\frac{26-2\cdot5}{2\cdot5}=9\cdot4$ gives the atomic weight of phosphorus. It was probably by similar reasoning from a still more inaccurate experiment than this one that he obtained the number 7·2.

Sulphur, which stands in the first Table (of 1803) at 14·4, was altered in the list published in the 'New System' to 13. These numbers were derived from a consideration (1) of the composition of sulphuretted hydrogen, which he regarded as a compound of one atom of sulphur with one of hydrogen, and (2) of that of sulphurous acid, which he supposed to contain one atom of sulphur to two of oxygen. Dalton knew that the first of these compounds contained its own volume of hydrogen; and he determined its specific gravity, so that by deducting from the weight of one volume of the gas that of one volume of hydrogen he would obtain the weight of the atom of sulphur compared with hydrogen as the unit. The specific gravity he obtained was about 1·23 (corresponding nearly, he says, p. 451, to Thenard's number 1·23). Hence (as he believed air to be twelve times as heavy as hydrogen) he would obtain the

atomic weight of sulphur as $(12 \times 1{\cdot}23) - 1 = 13{\cdot}76$, which number, standing half-way between 14·4, as given in the first Table, and 13 as given in the second, points out the origin of the first relative weight of the ultimate particle of sulphur. So from sulphurous acid he would obtain a similar number: taking the specific gravity as obtained by him (part ii. p. 389) to be 2·3, and remembering that this gas contains its own bulk of oxygen (p. 391), he obtained $(2{\cdot}3 - 1{\cdot}12) \times 12 = 14{\cdot}16$ for the atomic weight of sulphur. As, however, we do not possess the exact numbers of his specific-gravity determinations, and as we do not exactly know what number he took at the time as representing the relation between the densities of air and hydrogen (in 1803 he says that the relation of 1 : 0·077 is not correct, and that $\frac{1}{20}$ is nearer the truth), it is impossible to obtain the exact numbers for sulphur as given in the first Table.

In reviewing the experimental basis upon which Dalton founded his conclusions, we cannot but be struck with the clearness of perception of truth which enabled him to argue correctly from inexact experiments. In the notable case, indeed, in which Dalton announces the first instance of combination in multiple proportion (Manch. Mem. vol. i. ser. 2, p. 250), the whole conclusion is based upon an erroneous experimental basis. If we repeat the experiment as described by Dalton we do not obtain the results he arrived at. Oxygen cannot, in fact, be made to combine with nitric oxide in the proportion of one to two by merely varying the shape of the containing vessel, although by other means we can now effect these two acts of combination. We see, therefore, that Dalton's conclusions were correct, although in this case it appears to have been a mere chance that his experimental results rendered such a conclusion possible.

XXXVI. *Action of Light on certain Vanadium Compounds.*

By Mr. James Gibbons. Communicated by Professor H. E. Roscoe, F.R.S.

Read November 17th, 1874.

Potassium divanadate in combination with organic matter is at first rendered green, and ultimately blue, by exposure to light, being reduced probably to the state of vanadium trioxide.

The salt is not sensitive to light in the absence of organic matter.

Gelatine mixed with potassium divanadate becomes slightly less soluble in warm water after being exposed to light. This is apparent by the unexposed portions of the film swelling and dissolving more quickly when treated with water than the exposed part.

If a colourless film of dry sodium orthovanadate (Na_3VO_4), free from organic matter, be exposed on glass to the sun for several hours, it only acquires a faint brown tint. The film kept in the dark for some hours, regains its normal colourless condition.

The salt does not undergo any change when exposed to diffused daylight.

Paper which does not contain any size of an animal origin, when coated with a solution of sodium orthovanadate, is darkened on exposure to light, the depth of tint depending on the length of exposure and on the strength of the solution used. The tint, however, never becomes darker than a slate-colour.

If the paper thus prepared be immersed, after exposure to light, in a solution of silver nitrate, the colour in the exposed parts instantly changes to a deep brown or to a

black, varying according to the amount of exposure. A tint of the decomposed vanadate which is of so slight an amount as to be with difficulty distinguished from the whiteness of the paper will, by immersion in the silver nitrate, be toned so as to become very perceptible. It is evident that paper prepared in this way might be employed for the purposes of photographic printing.

The unexposed parts are converted, by treatment in the silver bath, into yellow silver vanadate. This substance may be dissolved out either by ammonia or by sodium hyposulphate. This act of fixing changes the dark brown or black parts to red; this may be prevented to some extent by using a bath of ammonio-silver nitrate with an excess of ammonia, instead of the simple silver nitrate bath. The developed print can afterwards be toned with gold chloride.

The length of exposure required to produce a deep black is about one hour to a strong sunlight—this by using a solution of the sodium orthovanadate containing about 11 per cent. of the salt.

Some ligneous substance only must be present with the sodium orthovanadate for the production of the above-mentioned slaty tint; for if an albuminous body be present, a faint brown tint is produced after exposure to light, and the silver nitrate is not afterwards reduced to any very great extent.

The slate-colour of the reduced salt appears to be due to the formation of vanadium trioxide.

If the exposed paper be kept for some weeks, its colour changes to that of a yellowish brown, free vanadic acid appearing to be produced.

Gelatine impregnated with sodium orthovanadate, exposed to light and afterwards dipped into a solution of silver nitrate, becomes insoluble in hot water.

Silver orthovanadate is capable of forming a photo-

graphic image which is nearly latent, and which may be developed by the ordinary ferrous developer used in photography. To produce this image, two or three minutes' exposure to sunlight is required. To develop it, it is essential that little or no silver nitrate be present; otherwise the exposed and unexposed parts are reduced indiscriminately. The washed silver vanadate can be mixed with a solution of gelatine containing a little albumen spread upon paper and allowed to dry; it can then be exposed to light and afterwards developed.

XXXVII. *On Basic Calcium Chloride.*

By Harry Grimshaw, F.C.S.

Read November 17th, 1874.

When a strong solution of calcium chloride is boiled with calcium hydrate, the solution filtered and allowed to cool, a salt separates out in long, slender, needle-shaped crystals. This salt is called, in Gmelin's Handbook, hydrated chloride of calcium and lime, or hydrated tetra-hydrochlorate of lime, and, according to him, has been noticed by Bucholtz and Frommsdorf and by Berthollet, and analyzed by H. Rose, who found the formula $3\,CaO,\,CaCl + 16\,Aq$, or, in present notation, $3\,CaO,\,CaCl_2 + 16\,H_2O$. This is not a very simple or intelligible formula; and moreover the percentage of water found, which was 49·084, is considerably lower than that required by the formula, namely 50·793. The salt was therefore prepared and analyzed as follows.

The solution of calcium chloride was prepared by dissolving, by heat, pure white marble in moderately concen-

trated hydrochloric acid until saturated. This was boiled with an excess of milk of lime for about an hour, filtered whilst hot, and allowed to cool. The salt separated out on standing, in slender, white, needle-shaped crystals, generally from one half to an inch in length. They were sometimes obtained not more than a quarter of an inch in length and almost transparent, the separation not taking place until the solution was agitated. The composition of the crystals was in all cases the same, and was not affected by the length of time they were allowed to remain in contact with the liquid.

The crystals, dried as quickly as possible between blotting-paper, on analysis gave the following results:—

(*a*) 0·677 grm. gave 0·273 grm. Ca O.
0·442 „ 0·228 „ Aq Cl and 0·0012 Aq.
0·307 lost on heating 0·152 grm.

(*b*) 0·359 grm. gave 0·144 grm. Ca O.
0·794 „ 0·2045 „ Aq Cl and 0·0245 Aq.
1·052 lost on heating 0·52 grm.

Calculated for $CaO, CaOHCl + 7H_2O$.		Found. (*a*)	Found. (*b*)	Calculated for $3CaO, CaCl_2 + 16H_2O$.
Ca	29·144	28·80	28·77	28·22
Cl	12·932	12·86	12·794	12·522
O	5·829			8·465
OH	6·193			
H_2O	45·901			
Loss on heating	49·179	49·40	49·47	(H_2O) 50·793
	100·000			100·000

The constitution of the salt is therefore expressed by the formula $O\begin{cases} CaOH \\ CaCl \end{cases} + 7\,H_2O$. On heating, two molecules of the salt decompose, forming $3\,CaO + CaCl_2 + 15\,H_2O$.

The difference of the proportion of the constituents according to the above formula, and according to that assigned by Rose, is therefore that corresponding to one atom of water more or less. As the amount of water is nearly 50 per cent. of the whole, this constituent will exhibit the greatest difference, namely 1·614. Accordingly, the numbers above will be found to agree with the formula $O\left\{\begin{matrix}Ca\,OH\\Ca\,Cl\end{matrix}\right. + 7H_2O$, which loses on heating 49·179 per cent. The number found by Rose (49·084) agrees very nearly.

The salt is perfectly stable for any length of time if kept out of contact with the air. It may be also kept unaltered in the mother-liquor for some time. In the air it decomposes, absorbing carbonic acid and water. Over sulphuric acid *in vacuo* or in air, or over quicklime, it parts with a portion of its water of crystallization. Both these circumstances interfere with the exact drying of the salt.

With water it decomposes into calcium hydrate and calcium chloride:

$$2O\left\{\begin{matrix}Ca\,OH\\Ca\,Cl\end{matrix}\right. + 2H_2O = 3Ca(OH)_2 + Ca\,Cl_2.$$

By the substitution of hydrobromic for the hydrochloric acid in the preparation of the salt, I expect to obtain a corresponding bromine compound.

XXXVIII. *A Study of Peat.*—Part I.

By R. Angus Smith, Ph.D., F.R.S., F.C.S., &c.

Read March 30th, 1875.

When living for a short time on the edge of a peat-bog at Loch Etive, and speculating on its age and the possibility of learning from its thickness and condition the era of some remnants of human industrial efforts, I came to the following conclusions, published by the Society of Antiquaries in Scotland in 1870–71.

Before quoting myself, I may remind the Society that Mr. Binney had long ago brought forward to this Society some interesting observations concerning peat. He mentioned specially two examples, described to the Geological Society of Manchester (Trans. vol. iii. 1860) :—the first at Down Holland, near Ormskirk, giving out an oily liquid; and the second, with a brittle conchoidal fracture, bright as pitch, and giving no flame on burning, called "jelly peat." He traced its analogies with coal, and gave a theory of its formation. I cannot remember any thing very new in relation to peat since that time, although a great deal has been written as to its economic worth and management. A few years ago I began to form some opinions, of which I may give the origin.

Age and Growth of Peat.*

"Near the point we now are, the road divides into two: one line goes straight forward to Connel Ferry; the other leads along the hill-side to Ardchattan up Loch Etive. The plain between this and the Loch is a moss, called by Dr. Wilson the Black Moss. On pages 91 and 106 of his

* Proceedings of the Society of Antiquaries of Scotland, vol. ix.

'Prehistoric Scotland,' he says that stones showing the action of fire have been found, along with stakes which seem to indicate circular dwellings. He adds that they are under eight or ten feet of moss, under which is a foot of soil before coming to the gravel on which the cairns of the neighbourhood rest. This, he considers, indicates a period before the Romans. I suppose the question of the age of peat is quite unsettled. I am inclined to believe that a moss may be very old or comparatively recent. We scarcely give the peat-moss fair play, although in this case I do think the lower moss, being very black, must be old. When thinking of this subject, I turned to the 'Handwörterbuch der Chemie,' and found opinions there which it may be interesting to quote. An analysis by Liebig of a *Lemna* from a peat-moss in Switzerland is given; and his remark is mentioned, that the water in which it grew contained all the ingredients required for feeding it. It is inferred that one can easily understand why peat should grow under such circumstances on the most barren ground. When discussing the time required to grow, it is said that between Olching and Loehausen, near Munich, a surface of peat was burnt, and after sixteen years there was a depth of seven inches above the burnt part. In Erdinger Moss, in Bavaria, three feet of turf is found made in old cut-out beds. The newer turf is coarser than the old. 'In Langmoos an old road is covered $1\frac{1}{4}$ feet.' 'If the growth of turf is carefully attended to, it may become of great value.' Sprengel says that 'under favourable circumstances a peat-moss (Torfmoor) will produce more combustible matter than the best forest.' In Liebig's 'Agricultural Chemistry' he shows that wood- and meadow-land grow remarkably near the same amount of dry woody fibre—about one ton and one fifth per annum per acre.

"If we suppose an acre of ground to be covered with solid fir wood an inch thick, it would weigh about fifty-five tons;

and growing one ton and a fifth per annum, it would require forty-five years. Considering the rapidity of growth of surface-plants on some peat-bogs, we cannot suppose it less, when not removed by cattle or otherwise. Now, as peat will hold something like eighty per cent. of water, or even more, we may fairly allow in the same time five inches of peat to have grown, considered roughly equal to one inch of wood. On the other hand, we know that grass and other plants may be stimulated so that several crops may be obtained in the year—under favourable circumstances, let us say, three. We might thus have in some places five, and in others fifteen inches of soft peat in forty-five years, according as the peat-plants were ill or well fed. We must divide the peat into two classes (some purposes require more)—the fibrous (which is fresh or not very old), and the amorphous. The rapid growth can apply to the first only; the true black peat, burning with much flame, is much older, and we cannot yet calculate the time for its formation. In order to form it, a decay goes on, which consists of oxidation of the carbon mainly, whilst the (higher) hydrogen compounds remain and proportionally increase. The carbonaceous matters are carried away in considerable quantities in the brown water. This lost quantity is not easily calculated. Some peat-water has two grains in it, some less than one. If we supposed one grain per gallon, and thirty-six inches of rainfall, we should require to subtract 115 lbs. or about 1 cwt. per acre per year (and with seventy-two inches twice as much) for removal by water. When the peat is fibrous, there is no decay perceptible, and the growth may be taken without deduction. In some cases the plants remain nearly fresh, I believe, for centuries. In a lake-dwelling of which I am going to speak, the plants below the hearthstone and the floor appeared only slightly yellow. Still the decay is needful for the conversion to true peat. If the plant is well protected from air, and from flowing

water containing air, its change may be arrested completely. A certain openness of structure is necessary for the formation of true black peat.

"I need not speak of the matter, excepting in relation to the subject of antiquities. If these notions are correct, we may have from ten inches to thirty inches of open fibrous peat in a century, according to the nature of the water supplying the moss. If the supply of water is abundant and easy, and if the water itself is well supplied with earthy salts, we can see no necessary limit to the depth which peat may attain during long ages. If there be a plain, and if the plants are obliged to obtain their inorganic salts by capillary attraction or osmose, or by the dialytic action of the peat, then a natural limit must take place. It will not be a sharp line; but the early peat will grow very fast, and the later slowly, until it ceases to increase in depth. We must therefore judge very differently of different mosses. One may have remained of the same thickness for an indefinitely long time, being as thick in much of the prehistoric as in historic days; another may have grown about two feet in a century. This latter, however, will not, in all probability, have attained the black stage at which the richer hydrogen compounds are found. At present we have no guide to the length of time required for this stage.

"When people speak of the age of peat as being excessively great, they ought to mention the quality of the peat. If black, we may allow at present very great antiquity, until we learn better; if covered over from air, we may allow a great age, even to fibrous peat; if otherwise, there seems no necessity for speaking of a very great age. By following up this inquiry, which I know to be imperfect, but new, at least to me, we may arrive at some more definite ideas. As an extreme one, I may consider that less than a century cannot be allowed to thirty inches of fibrous peat, and

that where water is very pure, and the supply of salts obtained by dialysis only, the probability is that one foot in a century is plenty for moderately soft peat; but when the moss is deep, and dialysis has a great distance to act, growth may stop entirely. In seeking to grow turf it seems important not to cut down to the soil. The peat grows best on a portion of its kind; this is to be observed abundantly in places where part of the ground has been left bare and part covered. It is rather remarkable that, in questioning Mr. M^{c}Gregor (the ground-officer of Lochanabeich), his opinion came out that it would take about fifty years to grow a foot under favourable circumstances such as he had seen. If these ideas are correct, we may gain by the manuring of peat, if it is ever much required in the arts. As a scientific experiment, at any rate, it is well worth attending to. (*It will be seen that there was much to learn.*)

"As to peat obtaining its inorganic constituents from rain, I think it improbable, as in that case there is no limit to its possible thickness, whereas the thick deposits are all in low places, in hollows of hills, where superficial sources of the salts are more readily found."

Since the above was written, a socketed bronze spear-head was found at the bottom of the moss spoken of, by Dr. Dan. Wilson. The peat was certainly not so hard or brittle at the bottom as many mosses described. The weapon would, I suppose, point to a possible great distance of time, at least the early centuries of our era; but it produces no certainty of date, as a piece of bronze might have been lost long before the moss began to grow, and, on the other hand, the moss may for a long time have ceased to grow.

I might adduce instances where there is much more grown than I have said, were I to seek the experience and acquaintance of others.

If peat grows so rapidly, why does it not grow everywhere? I was told that at Cairnmonearn it was at one place cut

by persons who, having no knowledge of the subject, did not leave any of the vegetable surface, but levelled quite to the gravel, the result at present being that the place is quite bare. The practice of planting peat and growing it as a crop is therefore already well known to many persons; and the careful landlord who insisted on pieces being left behind, has left for his successors a valuable field of fuel in a place where nothing else can grow, whilst the careless one close to him has rendered the land a desert. Now this is the very thing that I wish first to call attention to.

It is at present the opinion of many that the only thing to be done with peat when it cannot be economically compressed, is to throw it or have it washed away, if not burnt away, so as to make land for richer crops. In places where richer crops will grow, this may be right; but there may be some of even these places where it would be wise to cultivate certain portions for fuel. The use of coal will probably always be more expensive than the use of peat in many parts of Scotland and of Ireland, if not of England; and the power of growing fuel on land that will grow nothing else well, is a great and valuable power. Systematic peat-cuttings have been common enough; but systematic peat-growing has been rare, and the circumstances best suited have not been studied.

The ash of peat varies, it is said by Websky (Jahresbericht über die Fortschritte der Chemie, vol. xvii. p. 804), from 0·5 to 50 per cent. I know of no such wide numbers myself, and shall be inclined to keep to the amount found at Loch Etive, viz. 6 to 10 per cent. calculated on the dry matter, and 1·2 to 2 on the wet peat. The ash is chiefly siliceous; after silica, the earths are in most abundance; but alkalies and phosphates are in very small quantities.

Fuller analysis will follow.

For the growth of a peat-bog it is required, from the foregoing, that the water and plants should break up rocks

to the extent, in some places, of about one tenth of the dry matter of the peat. The supply of inorganic matter must be brought by water; and accordingly we find all peat-bogs which grow to a great depth to be in the midst of a very abundant supply.

Frequently they receive the drainage from a hill, so that all the inorganic matter washed down seems to give the peat the first choice. The organic matter must have its supply of nitrogen, and the abundant rain of the peaty districts never fails to contain some ammonia or some nitric acid.

Peat, then, grows where water is constantly bringing it food; and the inorganic matter is easily got even in soft-water districts. One grain of salts in 100,000 would be 70 tons per annum on 1000 acres, with 70 in. of rainfall, and enough to grow 700 tons of dry peat or 3500 of wet peat. I have no experience of peat on soils with hard water.

If the supply of water is small, there is a small supply of peat; and this we see on places where there is no stream from above; but it sometimes happens that there is a considerable amount of peat on land which occupies the upper part of a rising ground, having no supply of water from a distance at least: in this case there may be from three to four feet—as an instance, at the stones of Callernish in Lewis, where there is said to have been this thickness. This I suppose to depend on the capillary attraction, as well as on the existence of broken ground or fine earth or very soluble rocks. When it reaches a height not manageable by capillary attraction, the peat, I believe, almost entirely ceases to grow, being fed only a little by the rain, or in some places by spray.

The bogs of Ireland are said to rise as much as 40 feet, sloping down very slightly on all sides, and occupying a great plain. The distance necessary for the water to run facilitates the diffusion of salts.

On isolated pieces of rock I have seen peat grow to a thickness of from 1 to 2 inches; but there is no power to go higher, apparently. In instances to be alluded to, the field of peat, when large, is said to grow 12–15 feet above the water-level; the maximum depends on many conditions. Such large masses are never dry, although narrow districts are readily drained.

The amount of inorganic supply from the air must be small. If it were desired to increase the rapidity of growth, the mode of acting would, I suppose, be the same as is adopted in all cultivation, and soluble salts, or salts not so insoluble as the hard rocks so often found in the peaty districts of our island, would require to be used. It is not improbable that a very large crop of fuel might then be grown.

And now, some one will say, why adopt this fantastical idea? Is it not better to clear the country of swamps, remove peat-bogs by improving the drainage generally, and raise the best crops we can?

It must not be supposed that a peat-bog is a swamp in the sense used in southern climates: nor is it a marsh; it produces no marsh-fevers. The water itself is disinfecting, and the peat wonderfully preserving. The air over it may be damp, but little to be noticed in a damp country. The peat-bogs may be inconvenient as being soft (sometimes too soft to walk upon) during most of the year; they may be poor feeding-lands, producing little grass and sometimes no available grass; but we must still respect them as having long grown fuel for many of our countrymen, and promising to grow it much longer.

There are many parts of Scotland, certainly, where fuel is cheaper in this than in any other form, large parts of Ireland, and probably some of England, as well as of Wales. Is it probable that the time will ever come when coals will become cheaper in all these places? This probability has

surely ceased. The cheapening would require an extension of railway- and boat-communication which the scattered inhabitants of peat-growing districts cannot be expected to support.

Landowners need not, therefore, look too much to the outer world for fuel; but on hilly places and many other parts it seems their interest to keep up a system which has so far succeeded, and which the present aspect of things does not prove to be easily superseded. I certainly would recommend the careful preservation of a certain amount of peat-grounds, and also experiments on the mode of accelerating the growth.

Since a failure to obtain fuel would render the places spoken of simply uninhabitable, it is dangerous to presume much on any other source, and it is only wise, in my opinion, to retain the supply which is known to be lasting. The supply of peat is sometimes said to be inexhaustible. It is not so; the supply of a district is for the farmers around very frequently easily exhausted. It is only by fostering growth that any long cultivation can be expected of many parts; whilst there are some enormous bogs which are so far from habitations that it does not seem probable that there will be any benefit in removing them until surrounding conditions change. An advantage of the peat is, that it can be cultivated near one's house and used when required. The disadvantage is, that it cannot afford much carriage. It may be found to be the final and most convenient method, in all the northern part of this country, to have a field growing fuel at the side of others growing corn and potatoes; and it certainly is in some places the most convenient method at present. The habit, however, is going out more than is safe; and the mode of cultivation never has been sufficiently systematic.

So far as I know, there never has been any attempt to assist or to stimulate the growth, either by irrigation in

cases where the peat has not had its share of the floods, or by manuring, and so hastening its increase.

Notwithstanding the limited sphere which seems assigned to peat, there is to be found in books new and old a great hope of using up the peat-bogs when coal is done, and of having an almost equal supply. The present supply, if depended on for the whole community, would be necessarily expensive and short in duration.

If we cut a ten-foot bog, we may allow by shrinkage one fifth to remain dry, and much less than this for its equivalent in bulk of coal—five times according to Prof. Johnson. Half a foot of coal at the surface would certainly be more valuable than 10 feet of wet peat.

The district, including as much as a man can walk over to his daily work, would soon be cleared, and a migratory population would continually demand new arrangements for living and for carriage of products.

Whilst, therefore, I speak of the value of peat, it is only under limited conditions, as well as in limited districts, so far as the country during its coal-using period is concerned.

I was told in Argyleshire that about 50 years was a good time for recutting; and I was told the same thing in Kincardineshire. This looks like knowledge which has been gained by experience; at any rate it is a wide-spread theory not spread by books. The working people of the east and west of Scotland, north of the Clyde, have had little communication with each other, and are different in habits, in race, and in language.

In working a moss on the Dee, the peats are first taken away in barrows holding 18 peats. When these are dried, 40 of the barrows are a load for a horse; a load, then, is 720 peats. A family fire used all the year requires 24 of these loads, or 17,280 peats. The peat is 1 foot by 4×4 inches; or, generally, there are 9 in a cubic foot. The total required per annum is, then, 2160 cubic feet calculated

wet. This is a square space of 23 feet very closely, and 4 feet deep. But, at the rate at which the peat spoken of was grown, the amount on an acre annually would be 2454 cubic feet, considerably above that demanded; or if we take the observation of Sir Richard Griffith, one third of an acre would be sufficient to grow the quantity. Now we have a right to suppose this attainable by selecting or averaging circumstances suitable to the growth; and then we may have greater depth and less surface still.

The elements of the calculation differ very much; but we may say that the value of the peat in the cutting alluded to, coal being 20*s*. a ton (a low estimate on the hills), is from £4 to £5.

I was told that the loads were heavy: let us call them 12 cwt., which would be enough for the hills; we should then have 24·4 tons of peat (often at 25 lbs. weight per cubic foot) burnt per annum; this is quite a low weight. If an equivalent of coal were used, about 5 tons would be wanted; and it must be in few parts of the Highlands that they can be got for a pound a ton. It would therefore be needful for the cotters to spend, at a very low calculation, £5 to have the same comfort; indeed, when they use coal, they generally half starve themselves. In Manchester, people who keep only one fire, require about $4\frac{1}{2}$ tons in a year at 20*s*. a ton, or nearly five pounds; and in the Highlands it is quite fair to suppose that more would be needed. Nay, it is not a supposition, because it is required if not obtained; the climate is colder and wetter, the summers are shorter, and there is more heat required merely for drying clothes. However, the sum of £5 is too low for the 5 tons required; but it is high enough to prove the point before us. If a man got £5 for every acre of poor land he cultivated, he would, with a very moderate-sized farm, soon feel rich; for some acres he would be pleased with 5 pence. We may leave out altogether any inquiry into the value of land in

most cases, and say that an acre of land would be very well set aside for a family for fuel, and that it would produce more value than an acre of land used for other crops in very wet districts and on cold hills.

It would, of course be needful to inquire into the best positions for peat-growing. At present we only know it generally; but we know enough to begin successfully.

It has been desired to show that even on fair land one acre might be preserved for a small family; but if the family had several apartments with fires, more would be wanted. On the hills and among sheep-farms, counting by thousands of acres, this is really nothing; in lowlands in outlying districts it can still have calculation in its favour. It must not, however, be supposed that if a few acres pay well, a few more will pay also. It is economical in the case in question to cultivate only for the neighbourhood. If more is attempted, cartage will cause a loss.

In short, I advocate a systematic peat-agriculture in the north carried on so as to have easy carriage to every house far from railways and deprived of access from any cause; and I have reason to believe that even in this age many would be great gainers, whilst the gain will increase as coal becomes dearer.

Growth of Wood.

But some one will naturally ask, why not grow wood? This question has already been partly answered; but it may be somewhat further examined. I wish to show that peat will grow in places where it is extremely difficult, if at all possible, to raise wood, and, next, that the growth of peat is more rapid than the growth of wood. For the prevention of excessive floods nothing is better than peat, on account of the enormous amount of water it retains; it is a natural reservoir and a forest at the same time.

As to the first point, the difficulty of raising wood. It

is well known to all who have gone much to the hilly parts of Scotland and Ireland on the sea-coasts, and, we may say, England also, that the trees seem to avoid the sea-breezes even at a considerable distance from the shore. They lean from the sea even in a great deal of Lancashire; and on the northern sea-coasts, if defended by a wall, they rise up as high as the wall, and then appear at a distance as if sharply cut off by a hatchet.

There are, however, sheltered places even near the western Atlantic where trees grow well; and in these let wood be grown, for beauty as well as for use. So valuable and beautiful is a forest, that it would be wise to grow wood, even if fuel could be had at a much cheaper rate. Wood has uses so numerous, that it will not be attempted for a moment to argue as if peat could ever be an actual rival, except for a limited purpose and within a limited range.

For want of wood, I have long been inclined to think, the great empires of the Euphrates fell, perhaps even Egypt, Syria also; and it is very clear that Asia Minor, Greece, Italy, and Spain have all suffered: some of them would of necessity have ceased to be powers in the world, if for no other reason than this want of heat so important for the numerous demands of civilization. I have even come to the conclusion that Germany owes as much to her forest-cultivation as to any one individual fact in her economy. She has treasured her strength, and her people have been kept in comparatively comfortable houses, although even she would have done well to have treasured it more. Had we not been possessed of coal, our neglect of forests would have been fatal to us. How long a dried-up and burnt-up land, as much of Spain is said to be, must wait before it has its forests restored, must be the painful experience of millions to come.

We have burnt up our forests; and with us the planting, inclosing, and care are a great expense. Nature seems

during seasons of neglect to make it up to us by the growing of peat; and now let us see how rapidly it is grown compared with wood.

In some places you will be told by gentlemen, by the ministers of the parish and some of the farmers, that the peat is almost as dear as the coal. This of course depends partly, sometimes entirely, on the carriage; but there are other cases where, I believe, the population would starve without peat, or be reduced to the state of the dwellers in Terra del Fuego, civilization becoming impossible. Many persons who have to pay high wages to have their peat cut, take the readiest combustible—coals, as they are convenient, if a little dearer; but by many, and by all the poor, the fuel can be dug economically, the latter using their own hands in districts where the people, as a rule, have a good deal of time at their disposal. This quality of being within easy reach is never found with coal, and is not to be forgotten. The idea of buying peat or coal, with many would be preposterous. They have certain rights to a peat-moss; and all I advocate is the systematic protection of these mosses, these rights, and the cultivation of the fuel as carefully as they would cultivate the oats, although the crop may be slower sometimes, and to be reaped only by the succeeding generations; and my reason for doing so is, that I find fuel becoming dear in some parts of the Highlands, and a hitherto abundant supply of nature is turning into a serious expense.

In estimating the growth of wood, Liebig, as said, gives about a ton and a quarter to an acre annually. As it was better to obtain the numbers from a northern climate more allied to that on which the peat was grown, I have consulted Mr. John Begg, Junior, at Durris, Deeside, and obtained the following information:—

"I take on the Durris estate a wood 35 years old, and composed of Scotch firs and larch. (My reason for

starting at this point is, that the wood being half-age, the age of maturity being generally 70 years, the amount of wood or thinnings taken out and dried previous to 35 will, I believe, correspond to what would be taken out from 35 to 70.) The number of trees planted at first would be 5000 to the acre; and at the age of 35 there would not be above 500 to the acre, and at 70 not more than 150. I find, then, that an average tree in one of our fair-growing woods, and, I should say, an average of the country, contains 644 lbs. weight or $7\frac{1}{2}$ cubic feet of timber, 84 lbs. of brush or branches, and an equal quantity of roots, in all 812 lbs., which, multipled by 500 trees, gives 406,000 lbs. of wood to an acre.

"I presume a deduction of one fourth would be the weight in the dry state. On looking at Brown's book on Forestry, he says that at Castle Forbes he found Scots-pine wood to weigh in the rough state, at 60 years old, from $81\frac{1}{8}$ to $83\frac{1}{6}$ lbs. to the cubic foot..... It has long been found that wood between 35 and 60 years old is about the same weight, and after that it gets lighter. I have found spruce wood to weigh at 35 or 50 years 70 lbs. per cubic foot, and at the age of 90 only 57 lbs.

"Hay 1 ton to 3 tons to an acre.

Oats { straw 2420 / seed 1600 } 4020 lbs."

I scarcely know what data for weight to adopt. I intended to take 33 lbs. for the dry wood; but Mr. Begg insists on 50 at least for his district; and after all it is not of much importance for our case. Neither do I know the shrinkage; but it is evident that the weight of the wood is great at Deeside, although it is not so great on the heights. Karmarsch's table, quoted in the 'Handwörterbuch der Chemie,' gives the specific gravity of the *Pinus sylvestris* green as 0·811 to 1·005 (this last, although above the weight of water, is not so high as Mr. Begg mentions).

It gives larch as 0·694 to 0·924; the weight of the air-dried pine 41 lbs. per cubic foot, and of larch 30, Hanoverian measure. The Hanoverian pound is larger than ours, and the foot less; and we have 51·8 lbs. to a cubic foot of the pine, and 37·9 lbs. for the larch. If a cubic foot weighed 86, and shrank down until an equal space weighed 50, we should have a loss of 42 p.c. without allowing for shrinkage. We shall not be far wrong if we call it 50; and this makes 203,000 lbs. an acre in 35 years or 2·5 tons per annum of dry fibre, equal to about 2 tons of coal. This is not an amount that must everywhere be expected.

Let us compare this with the growth of peat on the same estate. We have in 44 years 30 inches, equal to 108,900 cubic feet on an acre; of this every cubic foot weighed 57·8 lbs., which dried to 11·56. We have, then, in 44 years 1,252,350 lbs. of dry combustible matter, or 559 tons, or 12·7 tons per annum, which being taken at the value spoken of, nearly $2\frac{1}{4}$ of peat to one of coal, is equal to 5·6 tons of coal. In this calculation there is no hypothetical quantity.

This certainly is a marvellous result. The amount obtained appears to be less by my estimation than by taking the figures spoken of in the extracts to be given; but we appear to come to the conclusion that in a wet and cold climate the best mode of growing combustible matter is by the cultivation of peat. The expense of growth must then be considered. The peat from which the data last given have been obtained was grown so high up that no trees were found; it was on land that was valuable for nothing but that by which it was occupied. The value in rent was scarcely any thing for grazing; let us say, fourpence an acre. There was no inclosure, thus saving one of the great expenses of wood-growing; there was no forester required to look after the trees; there were no rabbits, sheep, or cattle to kill the young or old; there was no interruption to

motion unless when the ground was very wet; and drainage would have been a disadvantage.

On the other hand, if systematic peat-growing were undertaken, not drainage but irrigation might be necessary; that is, it would be needful to choose a place where abundance of water could be had.

By selecting suitable land, which is generally very low-rented, it seems to be the case that abundance of fuel can be raised for the outlying population. There may even be much to spare; and in some districts a large surplus can be obtained. I shall not go into the question as regards the town population, as it is not now that our large towns require to think of this; nor shall I speak of the low grounds where rich land for grain is to be had; but my first object, as stated, is to show that peat is really an important *growing* fuel, and that it is not wise to root it out as is so frequently done, but that it is wise to preserve a certain portion for the near inhabitants, as it can be raised at a price far lower than coal or wood, the land costing almost nothing, and the labour very little. If care is not taken to have the peats grown near to the spot where they are used, the expense becomes great. I am told that the cutting, drying, and stacking of peat in one district where much is used, amounts only to 1*s.* 9*d.* per ton on the dried product. If we multiply this by $2\frac{1}{2}$ we obtain 4*s.* 3*d.* as the price of $2\frac{1}{2}$ tons of peat, equal to 1 ton of coal. The 5 tons of coal, or its equivalent in peat, is obtained for 21*s.* 5*d.*, a price that would be low for one ton only of coal in nearly all the hilly parts of the country, and in most even of the coast-towns. I am aware that this price is not everywhere the same; but I had it on good authority that it is done where the work is systematic and the wages not low. I know that some estimates are much higher.

I can quite imagine a peat-farm of no excessive size growing fuel as fast as it can be made use of by itself and

neighbours, or, if compression should succeed, growing also oil and perhaps ammonia, when that can be economically removed. But care would be required to have the proper plants and to feed them properly; and that is not, so far as I see, to be expected in the immediate future, without some previous cautious experimenting.

Collected Observations.

As it was not my intention to write so much as I have done on peat, I had got this length before I examined its literature; and perhaps it may be as well to introduce it here, so far as it seems to be necessary. It is not easy to exhaust such a subject; and I shall attempt only to give certain specimens. And first a little must be said of the etymology of the words *turf* and *peat*.

Beckmann quotes various authorities, saying that at certain distinct periods the use of peat was discovered in certain places in Germany and France. I think it clear that the use is far older than history—in Germany, at any rate; and when it is said that its value was found by any named person, it means probably that the wood had been exhausted and the people had taken then to burning peat instead.

I prefer the name *peat* although *turf* is an old German and English one; but it is confusing, as it means also a grassy sod. The number of compounds beginning with *Torf* in Kaltschmidt's Dictionary is 39, besides those that end with the word; so that it has attained great importance, as indeed the substance had even in Pliny's time.

I do not doubt that our word *peat* is the Low German *Pütte*, of the High German *Pfütze*, and that it has come from the Chauci or their neighbours straight to us, and probably was the name used by them when Pliny wrote. It means swamp, standing water, and well, and has the same origin, if, indeed, we may not call it almost the same

word as *pit*. The verb *pfützen*, to dabble in water, or splash in water, correctly points out the old process, and even the newest, which by machines resuscitates the old. Indeed, by every plan there is a good deal of dabbling in the soft muddy-looking material. In Scotch and Irish Gaelic, *monadh* and *moin* (equal to mountain, I suppose) are used as if the idea were that the peat was merely a part of the hill, and the greatness and profusion were thus indicated. It might also lead one to suppose that, after burning the trees, they had taken to burning the hill itself on which they grew.

The word *turf* seems to have a similar origin; the verb most nearly approaching to it is *torfa*, "*graben*, to dig," as given in Heyse's Deutsches Handwörterbuch. From this probably comes the common Latin word *turba*, which I believe is not an old or classic word. I shall leave others to trace it into Asia.

Heyse thinks the original is the Low German form of the High German *Zurb*, a broken-off clod or sod, connected with the verb *zerren*, to tear.

Cleasby and Vigfussen's Dictionary does not give *Torfa* in the sense of a verb *graben*, and indicates that the Icelanders learnt the use of peat from Scotland.

A little volume exists called 'Dissertatio physica de Turfis,' to the title of which is added "sistens historiam naturalem cespitum combustilium qui in multis Europæ regionibus et præcipue in Hollandia reperiuntur ac ligni loco usurpantur;" it was written by Joh. Hartm. Degner, and was published at Utrecht in 1729.

Degner describes the chief points relating to peat, beginning with the derivation of *Torf*, which he makes little of. He then treats of the districts of peat; and when he tells us of the great moors of Germany, Holland, &c. we have it put prominently before us that it is not in the Britannic islands that this fuel is preeminent in quantity.

In the third book, "on preparation of peat," we are told of the usual mode of cutting and also of the mode of taking out the fine peat from the wet places, ponds, and ditches, by means of nets. The magistrates decide in what places peat shall be taken, as otherwise not only the fuel of the neighbourhood may be interfered with, but the drainage and embankments. It would appear that the modern mode of breaking the peat fine and taking it out of the water in a condition resembling mud was an old one. The texture is by this means rendered less open. The more it is kneaded and pressed the more dense and solid is the peat. Patents have lately been taken out for doing this, as well as for methods of doing it.

"XI. Præcipua in labore hæc circumstantia est observanda, ut materia turfina, tanquam massa pistoria bene misceatur et subigatur; quo magis enim depsendo compingitur, eo solidior et densior sit turfa, eoque meliores et ignem diutius foventes largitur carbones. Cum contra, quando materia minus bene subacta sit, turfas porosas, spongiosas, non diu cohærentes, sed cito dissolubiles inque igne deflagrantes sistat."

The labour of digging or, rather, fishing out the peat is so great that the inhabitants of Holland will not do it, but bring yearly several thousand strangers from Hanover and Luneburg, who work with wonderful patience.

It was the custom where the levels unite to sell off all the peat and then to use the land for agriculture. It is said in XXVII. that this plan of obtaining good land was first discovered in the 15th century, as told by W. van Gouthoven, 'Chronick van Holland,' p. 574.

The 4th chapter is on the differences in peat, which the author fully recognizes.

In another place he shows the difference between peat and turf, which he names "turfa" and "cespes," and which it is better in English to call peat and turf. The

English as well as the Germans have used the name for both and caused confusion. § XV. cap. 11, he says "the turf (*cespes*) may grow to be cut again in 30–40 years."

In Chapters 5 and 6 he gives various opinions regarding peat, and discusses the question, "Is it rotten wood?" in 7, "Is peat earth?" in 8, "Is it bituminous or sulphurous?" in 9, whether it is a marine excretion; in 10, "Is it vegetable?"

In Chapter 11, he recognizes peat as a distinct growth, and also says that moss and marsh-grass grow abundantly in all those places where peat is abundant. It is added that it has been observed by botanists that the roots and stems of the mosses grow both in winter and summer, and press deep into the earth and water. The author thinks that a space two feet or more deep may grow sufficiently in ten, twenty, thirty or more years, so firm that men and flocks can pass over it, since mosses are during the whole year increasing in marshy places and yearly growing into dense masses. Not only does the moss grow, but the fossil part grows. He gives it as a decided opinion that peat grows, but requires a long time.

He gives the opinion of some old men that it would take a hundred years to grow as much as would allow it to be cut at the same place. (The cuts he speak of are deep.) Countrymen of a truthful character have said that they found at a depth of seven feet iron instruments, such as spades, and other abundant proofs that the peat had been cut before by their predecessors. And certainly, he says, if it did not grow, it must long ago have come to an end. He quotes Pliny, who tells (in Book xvi. cap 1) that they (the Chauci near Friesland) "take the mud with their hands, and drying it in the wind rather than the sun, warm with this earth their food as well as their entrails, which are stiffened by the northern cold." ("Captum

manibus lutum ventis magis quam sole siccantes, terra cibos, et rigentia septentrione viscera sua urunt.")

It has sometimes been proposed to throw the sewage of towns on to peat-bogs, and so procure disinfection. How this would suit has not been tried; it is extremely probable that the richer manures (that is, those much richer in nitrogen and phosphates than the peat has been accustomed to) would cause the growth of the mosses which constitute the material of most of the bogs of which I speak to cease, and bring out grasses or other substances more fitted for the feeding of cattle. Alkaline manures would probably suit best. This would be no misfortune in itself; but it would at least put an end to the plan begun, namely the disinfection by the peaty matter, after a time.

The manuring of peat-bogs by sewage so as to grow peat would not be hopeful, as we cannot look for any crop so extravagant as to make such agriculture valuable; but the humble *Sphagnum* would probably be well pleased to have the poorer food which comes as drainage from land already manured either by sewage or other manures. At present the peat does very well with the washings of rocks most disinclined to dissolve or to disintegrate. I can give no opinion on the best mode of manuring the mosses so as to cause the increased growth of the same plants, further than said; but I quite expect that trials would find some fit substance, probably earthy manures with little of phosphates.

Achard (Crell's J. 1786, p. 391), by some experiments, very imperfect however, wished to prove that the three layers of peat which he observed at Hertefeld, a village in the district of Königshorst, some 25 miles from Berlin, were alike in quality fundamentally, but differed in the relative quantity of some of their component parts. He does not seem to have noticed resins or wax. In Ireland, as Mr. Moir Crane tells me, three layers are prominent at

the bog of Allen—first red, then brown, and lowest black. In the same volume of Crell, G. G. Ten Haaff explains how he found sal ammoniac sublimed from peat.

Du Mênil, in Trommsdorff's 'Journal' for 1826, gives only 0·36 p. c. soluble in ether and 0·75 in alcohol. This is from what he calls Baggertorf from the Steinhuder Meer in Hanover.

In 1810, in a paper by Du Mênil, there is this sentence:—"Peat must be able to grow under water, as the following passage from Schoock's works show:—'Amplius sæpe usu venit, ut turfofodinæ se demum prodant subductione aquarum, bituminosique cespites ibi effodiantur, ubi antea navigabatur aut aquæ stabant.' Mart. Schoockii, Tract. de Turfis. Groning. 1658, p. 29."—*Trommsdorff's Journal de Pharmacie*, vol. xix. p. 2, 1810. Here the filling-up of great spaces by the growing of peat is clearly observed and expressed.

Du Mênil gives the names of many plants found in the peat, and mentions Papiertorf, Sumpftorf, and Moor or Modererde, and Torfruss or the soot from peat, which, he says, contains sulphate of ammonia, which is used for the formation of sal ammoniac and sulphur.

The Germans have many names for the various peats and their products (asphalt, resin, and wax)—Rasentorf, Pechtorf, Moostorf, Heidetorf, Schilf- oder Rohrtorf, Holztorf, Meer- oder Tangtorf, Sumpf- oder Baggertorf, Modertorf and Streichtorf, Maschinentorf, and Presstorf, Wurzeltorf, Erdtorf. Then they have Hochmoore, Kesselmoore, Wiesenmoore, and Meermoore.

Hünefeld speaks (in Erdmann's 'Journal' of 1838, vol. iii. p. 456) of bread-shaped masses which contain resin to the extent of 16·8 p. c., the resin being formed in the course of 800 years from bread which had fallen into the moss.

M. Dejean, of the Agricultural Society of the Somme,

read a paper "On the mode of extracting and preparing peat in the provinces of Holland and Utrecht; and on the advantages which will result to the department of the Somme by the adoption of a part of the Dutch processes." See Annales de Chimie, vol. xxxiv. p. 224, An viii. 30 Prairial. At p. 256 he says:—"This object, so interesting to the department of the Somme, and especially for the poor, deserves the attention of the Central Administration. And as the peat (tourbe) is a product of the soil, an agricultural product, if I may dare to call it so, attention to it becomes naturally one of the duties of the Free Society of Agriculture established in that department."

I call attention to this extract to show that I am quite aware that I am not the first to look with interest to the production of peat as a national industry—the actual growing, independent of the utilizing, which has long been studied; but many inventions must be made again and again, until the fitting time for their use shall come. That the time has not come for a general use of this idea every one will agree; but it has come, and has never in our time ceased to exist, for a considerable portion of the community—which portion is, in my belief, very much disposed to neglect its privileges, and, we may say, also its duties to a great extent, because these are not seen. This paper of M. Dejean might very well apply now to some parts of this country, where so much waste of material is allowed; but there is this consolation, that, if there is waste for this generation by the mode of cutting peat, there is no real waste, as the material, if allowed to remain, soon repairs itself, and all loose pieces are cemented by growth.

In Erdmann's 'Journal für pr. Chemie,' vol. xxiv. 1841, at p. 275, there is a paper by H. Reinsch on peat; and at p. 277 he says that, by slow distillation of the etherial solution of the peat-resins, he obtained a distinctly crys-

talline powder; but he could not find it on a second experiment. And then, further, he considers the resins to be more allied to wax.

In the same volume, W. F. Fürst, of Salm-Horstmar, gives the result of his analyses for nitrogen. He finds by combustion of peat with caustic soda an amount of ammoniacal salt equal to 2·1 p. c. of nitrogen.

In Erdmann's 'Journal für praktische Chemie' for 1839, vol. xvii. p. 16, is an article by W. A. Lampadius on the peat (*Schwartztorf* and *Brauntorf*) of Eger in Bohemia. He quotes an examination of the subject made for him by Dr. Palliardi of Franzenbad, from which I extract the following (p. 18) :—

"In the second year after cutting the peat, a number of confervæ are to be seen collected on the brown water in the cutting. In the third year the vegetation is more complete, and duckweed shows itself on the surface. In the fifth year at last appear rushes [*Binsen*], cotton-grass, and reed-grasses of different kinds. These two form a substance firm enough to enable one to walk upon it. This covering is called *Kühwampen*. After ten or twelve years *Erica vulgaris, Vaccinium Oxycoccus, V. uliginosum, Salix repens,* and *Pinus obliqua* appear upon the peat-cutting. After thirty or forty years, if the place has not been drained, and no cattle have been fed upon it, they cut peat again in the place where they consider there was formerly a cutting. . . .

"The deeper the cutting is made the better is the peat which is obtained; the solid substratum here is granite. The thicker the layer of the peat the denser it becomes, because of the pressure, the more bitumen also it contains and approaches nearer to brown coal. Where the moss has been drained it becomes perfectly dry and changes into a dusty, poor, black earth (Moorerde) which is entirely barren, a proof that water and pressure are necessary for preserving

the bitumen, and that the bitumen is necessary for preserving the vegetable matter of the peat. In some parts of this moor are mineral wells, which bubble up very strongly and contain a great deal of gas. They contain also a good deal of Glauber salt, which is found around them in efflorescence in warm weather. The stems and roots of pines or birches found in the peat are of a very dark colour, red and sound in the centre, and are used for fuel."

In Erdmann and Marchand's 'J. für prakt. Chemie' for 1839, in a communication from M. Reinsch on the peat of the Fichtelgebirge, the value of wood and peat are compared. When a klafter of wood is worth 6 florins, a klafter of surface-peat (Rasentorf) is worth 4 florins and 13 kreutzers (4·2 florins), and of hard peat (Pechtorf) 7 florins (6 florins 55 kr., properly; but 60 kreutzers are a florin). At the time when this was written the two qualities of peat were selling at the same price, namely a florin and a half. Peat was in reality much cheaper than coal. These prices have ceased in Germany; but the relative value of the heat-producers must remain the same.

Petzholdt, in the same year (Erd. J. ii. 1839, p. 314), says that peat-formation is more active in warm weather, and that the proportion of carbon is constantly increasing until there appears to be nothing but carbon. He adds also that, although the formation is rapid at present, some peat-bogs must be 2000 years old, judging by the substances or articles found in them. The escape of marsh-gas and carbonic acid he especially mentions as constant.

Several works or brochures have appeared of late years in Germany on peat. We have a useful history by Dr. Theodor Bromeis, Berlin, 1859, 'Die neuesten Methoden der Aufbereitung und Verdichtung des Torfs.' It gives the development of the pressure-system, wet and dry, of the mixture of peat with tar, and the machinery and arrangements used in various establishments in France,

Germany, and Switzerland. There is also an account of the use of peat in metallurgy and for locomotives.

On the heat-giving power, Dr. Bromeis says that peat, which usually (when air-dried) contains 34 p. c. of water, produces half the amount of heat produced by coke. Although compression cannot cause more heat to be produced, it removes 20 p. c. of water, and thus allows much more to be available; so that 160 of peat when thus pressed would be equal to 100 of coke: some, however, give higher values to the first (see p. 49 of Dr. Bromeis's pamphlet). But he also adds that "the peat may be made to attain the density of coal, and so become a more valuable material for heat. A cubic foot may attain the weight of 86 pounds." However, I do not wish to enter on the question of condensation, which has been sufficiently worked at by others. If the peat could be condensed for nothing or even cheaply, the condition of the question would change; at present the position of the bogs and the price of coal are too variable to allow of more than general statements.

Strange ideas have connected themselves with the growth of peat. Men have actually supposed that it grew in a mass like some great wen out from the ground, instead of simply growing on the surface like other plants. In allusion to this and to the two classes of peat, I may quote from three lectures by Albrecht Türrschmiedt "über Torffabrikation," Berlin, 1859. He says:—

"It requires no million years or geological periods to form peat: a life-time is enough to convince one that it grows—not from below, however, but from above. Still, if the arts in their haste expect in the course of seventy or a hundred years to find heavy peat formed, they will obtain no more by growth than the entangled mass which the sun bleaches and which gives flame but no heat. To convert mosses, heaths, grasses, &c. into that humous pitchy moss which you see in the beautiful specimen from Carolinen-

horst, and as Challeton makes it, requires not a life-time merely, but ages such as in ordinary speech we may call an eternity" (p. 18). I am more hopeful.

He quotes Prof. Scheerer as saying that the peat is a compound of, approximately,

60 per cent of Carbon,
2 „ „ Hydrogen,
38 „ „ Water;

whilst dried wood is

50 per cent of Carbon,
50 „ „ Water.

differing by 2 per cent. hydrogen and 10 carbon.

Challeton's method is to prepare the peat in the dry way. The Haspelmoor method, used at München and Augsburg, and introduced by Exter, is to heat dry and so cause the oleaginous and tarry substances to keep the mass together. The author takes an impartial view of the various methods, and is not willing without more trial to decide on the value of wet and undrainable bogs.

A little volume by Prof. August Vogel in München (1859), 'Ueber Torf, seine Natur und Bereitung,' goes into the description of apparatus for making and distilling with great detail. It is mentioned, by the way, that Sendtner, in his work on the vegetation of the peat-mosses, describes 332 plants as found in those of Southern Bavaria. Dr. Vogel considers that the decomposition is the same as with organic bodies decaying; but as oxygen cannot enter, the carbon is not reached: the changes are made at the expense of the oxygen of the humus; so that carbon is relatively increased. He thinks that it can easily compete with coal.

Dr. Dullo in his 'Torf-Verwerthungen in Europa' (1861), after describing the condition of the peat-industry, calls special attention to the cultivation of the moorlands of Holland and the previous production of the fuel. He says that 630 dollars (90 guineas) are demanded sometimes for

a morgen ($\frac{5}{8}$ acre) of peat of 5 feet depth. He thinks that one shilling for 1000 pieces is as much as ought to be asked for rent.

A great deal of information is to be found in 'Das gesammte Torfwesen nach den neuesten Versuchen und Verfahren, von Wilhelm Leo, Bergmeister und Mitglieder der Sächs. Societät für Mineralogie, &c.' I shall confine the extracts to the growth of peat (see p. 40):—

"The waters under the peat are the real producers of peat; and the growth can easily be forwarded by putting back the surface on the portion cut; in it the plants grow and the bitumen is preserved from the influence of the oxygen of the air. In the large peat-bogs of Bavaria a growth of 2–3 feet has been observed in 40 years, in Oldenburg 4 feet in 100 years, in Denmark only $2\frac{1}{2}$ feet. On the other hand, in Switzerland old peat-holes could be cut again after 30 years, having grown in that time 5 feet thickness of peat. This growth may also be improved by strewing the seed about. The water must move slowly over the surface, and not shut out atmospheric oxygen; if the latter occurs, vegetation ceases and decomposition begins. If, however, the surface is gradually raised out of the water and sometimes dried, the reed-grasses begin to be dried up, and other plants take their places, such as bilberries, heather, St. John's wort, mare's tail, and bog-moss, and at last it will bear light woods, such as willow, alder, and birch.

"Bogs grow on any place where there is standing water not drying in summer: all peat-bogs have been smaller or greater collections of water, some of them lakes." By the advice and under the management of Forest-master Luschka, a great peat-bed was opened at Gundholzen, on the Lake of Constance, in 1804. In 1827 it was opened again, when $2\frac{1}{2}$ feet of peat had grown since 1805. A ten-acre field was opened at Gayenheim, in the same district, with a similar result. The new part is less solid and of

lighter colour than the old (p. 42). The rootstock of *Arundo phragmites* is abundant in the young peat of Greifswald.

"It is known for certain that the part of the Alt-Warmbruch moss belonging to the city of Hanover has been cut twice over, and that where it was cut 30 years ago there is from 4 to 6 feet of new growth." He says that a moss may grow ten feet above the water-level. Perfectly dried peat, he adds, stands between wood and coal as a heat-producer.

'Rationelle Torf-Verwerthung,' by Dr. Ernst Schenck zu Schweinsberg and Dr. K. Karmarsch, being chiefly concerning the apparatus for preparing and using peat, I have not given it attention.

In 'Die Torfmoore Oesterreichs,' by Dr. George Thenius, p. 46, there is mention made of peat to be obtained after from 20 to 30 years either by "Stechen" or "Baggern," cutting or dredging. When dredging, as one may call it, is used, the peat must be in a fine state of division: this shows that decomposition of the fibre has gone to a great extent; the method cannot be employed for fibrous peat. It is added that the fact (that is, the rapid growth) has been proved in many mosses, in the Salzburg moss, the Bührmoos, and the Pass-Thurmoos, as well as other places. The author advises to retain the deep mosses and to remove the shallow by cultivation. There are many interesting points in this volume, and a description of new plans; but I cannot enter on the questions of machinery or price.

The German literature itself is extensive; and I cannot pretend to exhaust it here, still less to examine that of all our other neighbours. The English is even less known to me; but one of the earliest works I have found is from Scotland, by "William Aiton, Writer in Strathaven" (Lanarkshire), and published at the desire of the Highland Society. It is entitled "A Treatise on the Origin, Qualities, and Cultivation of Moss-earth, with Directions for converting it into Manure. Air, 1811."

In most treatises on peat there is an idea that it is a mass of broken vegetation and a fortuitous accumulation of rubbish. No one explains it, so far as I know, before Mr. Aiton, so well; he says, p. 25, "Moss-earth is nothing else than an immense collection of the successive crops of aquatic vegetables which have grown from year to year on the surface in humid situations and at a low temperature."

Although believing in the rapid growth of peat, he is not inclined to believe the account quoted from the Phil. Trans. of 1711, p. 296, that the Earl of Cromarty saw a field of old withered firs in 1651, and in 15 years saw them broken down and moss growing on the spot, which had become wet by the stoppage of drainage, and in 48 years from the first period saw peat cutting on the same spot for fuel. I see no reason for refusal to believe quite as much as this; and the quoted accounts would lead us to believe in even shorter periods.

Kneaded peat, Mr. Aiton says, is the best of fuel. He advises the making of peat-sheds to prevent exposure.

'The Natural and Agricultural History of Peat-moss or Turf-bog,' by Andrew Steele, Esq., of Crosswoodhill (Edinburgh, 1826), is an interesting volume, from which I draw a few sentences.

"I have myself observed the *Sphagnum* grow four feet in a year, springing from the bottom, and covering the surface of stagnant water of that depth. The *Hypnum fluitans*, which also grows in water, and floats on the surface, has been known to grow to two or three feet in length in a season, and to form a stratum of peat in 20 years" (p. 14).

"By replacing the surface-turf of a moss cut for fuel, I am informed, a peat-moss near Cupar Angus grew again so as to admit of being cut for fuel twice in fifty years" (p. 15).

"A great proportion of the mosses and mossy farms in the lowlands retain the name of woods, though there does not now exist a growing tree in their vicinity" (p. 27).

P. 27, he alludes to a noctilucous quality in peat-bogs, and to the mineral oils, naphtha and petroleum, found in most mosses.

The healthiness of people on peat-bogs is spoken of, p. 183 and elsewhere, p. 369. Dr. Plott has shown that intermittent fevers never occur in the neighbourhood of peat-mosses.

After speaking of the washing away of Blair-Drummond moss to make arable land, the author says, p. 297, "It seems necessary to remark that, as the agricultural properties of peat-moss are yet but little known, it would be rash to decide whether, in a future age, floating-off peat into the sea as a useless commodity may not, in whatever circumstances, be condemned. It is not impossible that, like the inhabitants of Holland, our descendants may consider peat-mosses as magazines of valuable manure as well as of fuel in those cold northern regions, too important to be squandered away with a lavish hand.

"Meantime it is not a little amusing to observe that while the improvers of Kincardine moss-grounds are floating-off their peats, others are exerting their best endeavours to catch them in their way from being lost in the sea, considering them as profitable manures; while the former are decrying peat-earth as a useless incumbrance to the ground, others are exulting in the discovery of it as a treasure; while some are despising it as a fuel as well as a manure, others in the same island are paying hundreds of pounds for the surface of an acre of peat-bog, believing that after it has served the purposes of fuel, its very ashes are the best of all restoratives of the vegetative powers of the earth." Artificial manures had not then appeared.

Visiting again the same district mentioned in the first extract, I made more inquiries and have come to the conclusion that, however various the growth of peat may be, there are cases where it is much more than the extracts I

first gave would show. I had the pleasure of visiting a part of Glen Lonnan with Mr. Duncan Clerk (Writer, Oban) and his brother. These gentlemen had a distinct remembrance of peat being cut in the glen forty-five years previously; both knew the place precisely and without any shadow of doubt; and we proceeded to dig in order to ascertain how much had grown since the last disturbance. We found 20 inches of clear peat before coming to the débris left on the surface after digging. This point is easily ascertained. We may be liberal in the calculation and not exact the whole 20 inches, but say that the rate was that of 3 feet in a century. The bog was soft, but still hard enough to allow us to stand or walk over, and would contain about 75 per cent. of water, as I know from experience of other places; but in this place I prefer to take the higher number often found and call it 80, leaving 20 per cent. of solid matter.

Further down the valley we found an old highland woman living entirely alone and far from neighbours. She called herself eighty-five years old; but it was clear that her mind had not suffered by time. She told us of the spot that was cut when she was ten years old; and I entirely credit her. The growth here was not measured by digging, but by comparison with the surrounding ground. It was about $3\frac{1}{2}$ feet, which is $4\frac{6}{10}$ feet in a century. It is sometimes unsafe to judge of the depth by the exposed side, because there are cases where the peat is so soft as to be pressed out from below; but then there is a gradual lowering or an incline, and no such thing occurred here: the sides were firm and kept their level.

My next observation was on the estate of Durris, belonging to James Young, F.R.S., on the Dee, in Kincardineshire. I was taken to the side of the hill Cairnmonearn by a man named Davidson, an intelligent cotter, who showed the spot where he had dug peats forty-four

years ago. This was measured, and found to be $2\frac{3}{4}$ to $3\frac{1}{2}$ feet thick to the bottom, to which point it had been dug, leaving only the upper turf for the continuation. This peat was of a light brown when dug up, but blackened when we were looking at it, and next day was very black. It was pronounced by all who saw it to be good burning peat.

It is not necessary to accumulate instances, or I could tell of persons who have cut peat twice in the same spot. A cotter at Dun MacUisneachan cut it twice in twelve years on Lochawe-side; but then the thickness was only a foot, a proportionally large growth; however, another, a comparatively young man, had cut it twice in fifteen years, and so on. Davidson tells me that at Durris, turf is considered fit for cutting in fifty years, as already mentioned.

I may gain here something from youthful reminiscences. I remember the peat-cutting on the north side of Lochawe forty years ago. There were many deep places very extensive also; but now they are quite level with the surrounding country, except on the parts very near the roads. The village of Stronmilichan had then many more inhabitants than now, and peat was the only fuel.

It is pleasant to give here extracts from the Reports of the Irish Commission, and especially the opinions and experience of Sir Richard I. Griffiths, Bart.

First Bog Report, or Report of the Commissioners appointed to inquire into the nature and extent of the several bogs in Ireland. (1810.)

Page 6. "It appears, from Mr. Griffith, that each of the four bogs included in the subject of his Report is a mass of the peculiar substance called Peat, of the average thickness of twenty-five feet, nowhere less than twelve, nor found to exceed forty-two, this substance varying ma-

terially in its appearances and properties in proportion to the depth at which it lies, on the upper surface covered with moss of various species, and to the depth of about ten feet composed of a mass of the fibres of similar vegetables in different stages of decomposition proportioned to their depth from the surface, generally, however, too open in their texture to be applied to the purposes of fuel; below this generally lies a light blackish brown turf, containing the fibres of moss still visible, though not perfect, and extending to a further depth of perhaps ten feet under this. In the instance exhibited in the Section at the close of Mr. Griffith's Report, are found small branches and twigs of alder and birch; but we do not understand him as being of opinion that such is by any means generally the case. At a greater depth the fibres of vegetable matter cease to be visible, the colour of the turf becomes blacker, and the substance much more compact, its properties as fuel more valuable, and gradually increasing in the degree of blackness and compactness proportionately to its depth: near the bottom of the bog it forms a black mass which, when dry, has strong resemblance to pitch or bituminous coal, and having a conchoidal fracture in every direction, with a black shining lustre, and susceptible of receiving a considerable polish."

P. 16. *Mosses.*—"Of the growth of moss in stagnant waters there are numberless instances; and in fact throughout the whole of the clay-lands in this part of the country, wherever water is suffered to remain any length of time, an incipient formation of bog (by the growth of aquatic mosses) is the invariable consequence.

"It has been supposed by some, that the whole of the country now covered by bog had formerly been forests, which having been either entirely or partially destroyed by fire, currents of water, or otherwise, the wreck of these forests falling in various directions had caused an interrup-

tion in the waters, and occasioned the growth of watery mosses.

"That some bogs may have been formed from such obstructions (as Hatfield Moss in Yorkshire) I do not deny, but that the bogs in this part of the country did not originate from such a cause is evident from the fact that trees or branches of trees are very rarely found in the interior of our deep and extensive bogs, but are always met with on the bog-edges, and on the edges of gravel hills or islands in the bog, lying horizontally and in no particular direction, frequently crossing each other, either attached to their roots or broken off; in this latter case, the stumps usually stand upright in the place where they grew, sometimes having six or eight feet of bog above them, and three, four, five feet, or rarely more below their roots.

"Trees are still to be found growing on the bog-edges, and in the valleys in the bog where the rivers flow. Thus, in the vale of the stream running from Lullymore by Lullybeg to Cuthaling (in Lullymore Bog), oak, alder, aspen, birch, willow, whitethorn, and holly trees are now growing; but I did not observe any fir trees, though they are found in the bog.

"It is probable that most people who have hitherto written on bogs, having merely examined the composition of the mass of bog near the edge (the interior being scarcely accessible), may have concluded that trees and branches of trees were as thickly scattered beneath the surface throughout the bog as in the part near the edge which had been examined.

"It is to be understood that I do not mean to apply the foregoing observations to the probable formation of bog generally, but merely to this division of the Bog-district, No. 1, which I have had an opportunity of minutely observing."

Fourth Bog Report. (1814.)

P. 112. "*Composition and Growth of Red Bog.*—The composition of the upper and lower bogs is exactly the same, and is similar to that described in my former Reports, particularly in the section of the turf-bank in Timahoe Bog, —that is, a regular gradation from spongy moss to a compact mass resembling pitch, or pitch coal. In the bog of Killcashiel (one of the upper bogs) I had an opportunity of observing the annual increase in height or growth of a bog for twenty years, which, in the spot where I noticed the fact, was about *two inches for each year*: this may seem an extraordinary increase; but the situation appeared to be particularly favourable to the growth of moss.

"Twenty years ago the water from a small lake situated close to the edge of the bog of Killcashiel was drawn off by a drain cut from the lake to a stream which runs beyond the edge of the bog; it was intended to improve the bog beneath the water, and that immediately adjoining the banks of the lake. The improvement was not followed up, and the drain was suffered in part to close. The bottom of the lake was now exposed to air; but the bog below remained still highly saturated with water. Moss immediately began to vegetate, and has continued to increase in height ever since.

"I was enabled clearly to observe this fact, from a turf-bank having been cut into the place where the lake formerly was; and, from the variety in the colour and texture, I calculated the annual growth of the moss, with nearly as much accuracy as the age of a Scotch-fir tree may be determined by observing the number of concentric rings visible when the stem is cut across."

P. 167. "The circumstance of the scarcity of fuel has induced Mr. Greene of Kilranalagh to stop the water in the drains, in part of a bog near his house, from which

turf has been cut nearly to the bottom; and Mr. Greene expects that the bog will grow again, and afford fuel to a future generation. The drains have now been stopped four years. I saw the bog last summer, and I found that in many places the whole of the drains, and many of the old turf-holes, were filled nearly to the top by a pulpy mass, which upon examination I found to be composed of matted grass and the bog-moss (*Sphagnum palustre*)—which when soaked in water has a gelatinous appearance, but when taken out and dried diminishes to less than a twentieth part of its apparent bulk in the water, and becomes hard and tough. Probably in eight or ten years the grass and moss will have grown so thick and firm as to bear a man to walk on its surface; and I doubt not that in less than forty years turf may be again cut from the old turbary."

P. 182. "It may be observed that the depths of mountain and lowland bogs are very different: the former rarely exceeds six feet; whilst the latter amounts to twenty on the average, and is sometimes more than forty feet. This may at first sight appear extraordinary; for if my argument be correct, it is certain that the shallow bog must have commenced its growth immediately subsequent to the formation of the mountains, and that the growth of the deep or lowland bogs was commenced at a much later period. But it should be recollected that moss will grow in some situations much more quickly than in others; and there have been instances (one of which I have mentioned in my Report on the district of the river Suck) of the growth of moss in a lowland bog amounting to two inches in a year for 20 years: this rapid growth of moss may perhaps be attributed to the great abundance of stagnant water which is always present in the lowland bogs, excepting in the hottest of the summer months.

"Mountain bog is much denser than lowland moss; this is the natural consequence of the difference in the rapidity of the growth of each."

A valuable little work on Peat and its Uses, by Prof. Samuel Johnson, of Yale College, must be quoted. On p. 11, Prof. Marsh is mentioned as saying "that he has seen in Ireland, near the north-west coast, a granite hill capped with a peat bed several feet in thickness. Many of the European moors rise more or less above the level of their borders towards the centre, often to a height of 10 or 20, and sometimes more feet." Peat islands floating are mentioned. The floating island of Derwentwater, however, did not, when seen long ago, appear to me to be peaty.

"Fibrous peat is found near the surface; and as we dig down into a very deep bed we find almost invariably that the fibrous structure becomes less and less evident, until at a certain depth it entirely disappears." "The ripest, most perfectly formed peat, that in which the peaty decomposition has reached its last stage, which in Germany is termed *pitchy peat* or *fat peat* (*Pechtorf, Specktorf*), is dark brown or black in colour, and comparatively heavy or dense.

"In Holland, West Friesland, Holstein, Denmark and Pomerania a so-called *mud peat* (*Schlammtorf*, also *Baggertorf*, and *Streichtorf*) is fished up from the bottoms of ponds, which on drying becomes hard and dense like the pitchy peat." This is, in America, called *swamp-muck*.

Prof. Johnson at p. 99, says:—"Two considerations would prevent the use of ordinary cut peat in large works, even could two and one-fourth tons of it be afforded at the same price as one ton of coal.

"The Nassau Water Department consumes 20,000 tons of coal yearly, the handling of which is a large expense,

six firemen being employed to feed the furnaces. To generate the same amount of steam with peat of the quality experimented with would require the force of firemen to be greatly increased. Again, it would be necessary to lay in, under cover, a large stock of fuel during the summer, for use in winter, when peat cannot be raised. Since a barrel of this peat weighed less than 100 lbs., the short ton would occupy the volume of 20 barrels; as is well known, a ton of anthracite can be put into 8 barrels. A given weight of peat, therefore, requires $2\frac{1}{2}$ times as much storage room as the same weight of coal. As $2\frac{1}{4}$ tons of peat, in the case we are considering, are equivalent to but one ton of coal in heating-effect, the winter's supply of peat fuel would occupy $5\frac{5}{8}$ times the bulk of the same supply in coal, admitting that the unoccupied or air-space in a pile of peat is the same as in a heap of coal. In fact, the calculation would really turn out still more to the disadvantage of peat, because the air-space in a bin of peat is greater than in one of coal, and coal can be excavated for at least two months more of the year than peat.

"It is asserted by some, that, because peat can be condensed so as to approach anthracite in specific gravity, it must, in the same ratio, approach the latter in heating-power. Its effective heating-power is indeed considerably augmented by condensation; but no mechanical treatment can increase its percentage of carbon, or otherwise alter its chemical composition; hence it must for ever remain inferior to anthracite.

"The composition and density of the best condensed peat is compared with that of hard wood and anthracite in the following statement:—

In 100 parts	Carbon.	Hydrogen.	Oxygen and Nitrogen.	Ash.	Water.	Specific gravity.
Wood	39·6	4·8	34·8	0·8	20·0	0·75
Condensed peat ...	47·2	4·9	22·9	5·0	20·0	1·20
Anthracite...	91·3	2·9	2·8	3·0		1·40

"In combustion in ordinary fires, the *water* of the fuel is a source of waste, since it consumes heat in acquiring the state of vapour.

"This is well seen in the comparison of the same kind of peat in different states of dryness. Thus, in the table of Gysser (page 97), Weber's condensed peat, containing 10 per cent. of moisture, surpasses in heating-effect that containing 25 *per cent.* of moisture, by nearly one half."

Composition of Peat—Analyses.

Prof. Johnson says, p. 43, "The average amount (of nitrogen) in thirty specimens, including peat- and swamp-mucks of all grades of quality, is equivalent to $1\frac{1}{4}$ per cent. of the air-dried substance or more than twice as much as exists in ordinary stable- or yard-manure. In several peats the amount is as high as 2·4 per cent.; and in one case 2·9 per cent. was found.

"The great part of the nitrogen exists in an insoluble or inert form; but, by the action of the atmosphere upon it, especially when mixed with and divided by the soil, it gradually becomes available to vegetation to as great an extent as the nitrogen of ordinary fertilizers.

"Under certain conditions, the free nitrogen of the air which cannot be directly appropriated by vegetation is oxidized in the pores of the soil to nitric acid; and thus, free of expense to the farmer, his crops are daily dressed with the most precious of all fertilizers." On p. 68 he says "When the night-soil falls into a vault it may be composted by simply sprinkling fine peat over its surface once or twice weekly, as the case may require, *i. e.* as often as a bad odour prevails."

I find it almost useless to make many ultimate analyses of peat, as I find that so many have been made; I shall give a collection of some.

In the Giessen Jahresbericht, vol. for 1871, p. 1089, we

read, "Osc. Jacobsen examined peat from the district of Hör, in Schonen. Remains of moss could be perceived only in the upper layers; the lower layers had a shiny fracture and contained some rather large pieces of wood. The denser part was of 1·07 sp. gravity; at 100 it lost 11·5 per cent. of water, and contained

Carbon.	Hydrogen.	Oxygen.	Nitrogen.	Ash.
51·38	6·49	35·43	1·68	5·02

"The ash contained

	per cent.
Potash	1·50
Soda	0·58
Lime	20·75
Magnesia	1·42
Alumina	6·60
Iron peroxide	17·34
Sulphuric acid	1·55
Chlorine	0·67
Soluble silica	6·50
Phosphoric acid	0·42
Carbonic acid	8·43
Sand and traces of manganese	33·50

"He found no paraffin in the peat by means of ether. Benzol dissolved more than ether. All the dissolved substances were resins containing oxygen. The part insoluble in alcohol and ether, but soluble in benzol, agrees in properties with Mulder's Delta resin. It melts at 70°, and contains 81·03 carbon and 11·98 hydrogen. From the hot alcohol solution a crystallizable resin may be obtained in groups star-formed, melting at 74° to 75°. The author agrees with Mulder that the resins are formed in the production of turf. This opinion is confirmed by the observation that from the lower and older stratum 3·26 per cent. was obtained, and only 2·5–2·7 from the upper."

I next give an interesting collection of analyses by Mr. George W. Hawes, as I wish afterwards to look at the relation between the plants which form the peat and the peat itself.

	C.	H.	O.	N.
Lycopodium dendroidum	48·79	6·62	43·15	1·44
—— complanatum	48·35	6·61	43·09	1·95
Equisetum hyemale.....	47·54	6·74	44·45	1·27
Aspidium marginale	47·18	6·35	44·30	2·17
Cyathea caniculata (section)	49·08	6·66	43·05	1·21
—— —— (cortical part)	52·04	5·26	41·18	1·52

The analyses are from p. 154 of vol. v. of 'The American Chemist' (No. 52).

A table of analyses taken from p. 24 of Johnson's 'Peat and its Uses,' shows the loss of oxygen in the formation of peat extremely well, beginning with *Sphagnum*.

	Analyst.	Carbon.	Hydrogen.	Oxygen.	Nitrogen.
1. Sphagnum, undecomposed.	Websky.	49·88	6·54	42·42	1·16
2. Peach-wood, ,,	Chevandier.	49·90	6·10	43·10	0·90
3. Poplar, ,,	,,	50·30	6·30	42·40	1·00
4. Oak, ,,	,,	50·60	6·00	42·10	1·30
5. Peat, light brown, porous, sphagnous.	Websky.	50·86	5·80	42·57	0·77
6. ——, porous, red brown.	Jaeckel.	53·51	5·90	40·50	
7. ——, heavy, brown.	,,	56·43	5·32	38·25	
8. ——, dark red brown, well decomposed.	Websky.	59·47	6·52	31·51	2·51
9. ——, black, very dense and hard.	,,	59·70	5·70	33·04	1·56
10. ——, black, heavy (best quality for fuel.)	,,	59·71	5·27	32·07	2·59
11. ——, brown, heavy (best quality for fuel.)	,,	62·54	6·81	29·24	1·41

upon which Prof. Johnson remarks:—"We notice in running down the columns that as the peat becomes heavier and darker in colour, it also becomes richer in carbon and poorer in oxygen. Hydrogen varies but slightly. Nitrogen is variable; but in general the older peats contain the most.

"Carbonic acid is the most abundant gaseous product of the peaty decomposition. Since it contains nearly 73 per cent. of oxygen and but 27 per cent. of carbon, it is obvious that by its escape the proportion of carbon in the residual mass is increased" (p. 26).

Although I believe this really happens, we cannot suppose all the carbonic acid to be formed without a supply of oxygen from the air as well as from the peat; and Prof. Johnson would no doubt say the same.

P. 43. "In several peats, the amount of nitrogen is as high as 2·4 per cent.; and in one case 2·9 was found." Although he adds that some are poor and contain less than half per cent., he does not give the quality of this peat.

Of nitric acid in weathered peat Prof. Johnson gives a table of analyses by Boussingault and Reichardt, in which the amounts vary from 0 to 0·421 per cent. (p. 44).

The ultimate composition of peat has been inquired into by W. Baer; and the results of his research, and his analyses of wood, are here given. The average of his peat agrees so well with one of the specimens examined in my laboratory that one may consider that the composition does not greatly vary from Scotland to Germany, but depends on the soil more than climate. The much larger amount of ash in ordinary peat is a disadvantage compared with wood.

The following comparison is by W. Baer, in the 'Jahresbericht über die Fortschritte der Chemie 1847–48,' p. 1112. The quantity of carbon is greater than in wood; but that of the hydrogen is less.

	Carbon.	Hydrogen.	Oxygen.	Ash.
Birch	48·89	6·19	43·93	0·99
Red Beech, 1st	46·10	5·79	46·87	1·24
—— ——, 2nd.	48·29	6·00	45·14	0·57
White Beech	48·08	6·12	44·93	0·87
Oak	48·94	5·94	43·09	2·03
Alder	48·63	5·94	44·75	0·68
Pine, young	50·62	6·27	42·58	0·58
——, old	49·87	6·09	43·14	0·68
	8)389·42	8)48·34	8)354·43	8)7·64
Average	48·677	6·042	S+N+O 44·304	0·955

	Carbon.	Hydrogen.	Oxygen.	Ash.
Peat	51·54	4·60	33·90	9·87
	50·13	5·36	35·24	9·37
	50·36	4·20	34·27	11·17
	53·69	4·84	31·73	9·74
	55·01	4·63	31·44	8·92
	5)260·73	5)23·63	5)166·58	5)49·07
	52·146	4·726	33·316	9·814

Specimens of peats from the Ledaig moss, Loch Etive, were of a moderately good quality, and gave results as follows :—

When dried at 100° C.

	No. 1.	No. 2.
Organic matter	90·563	93·520
Ash	9·436	6·480

consisting of

Carbon.....	52·721	48·961
Hydrogen	4·236	4·474
Nitrogen	1·660	0·957
Ash	9·436	6·480
Leaving for oxygen	31·947	39·128

Leaving out the ash, the composition is

Carbon	58·21
Hydrogen.....	4·68
Nitrogen	1·83
Oxygen.....	35·28

The result of Mulder's examination of humous bodies has not been to give satisfaction ; the probable reason is that the stages of decay of the woody fibre are very numerous. I am disposed to admire much that chemist's labours. It is agreed that there is oxidation of the woody fibre. Miller in his 'Chemistry' has put it simply thus :—

$$\text{Wood} \;..\; 4C_{34}H_{48}O_{22} + 6O_2 =$$
$$\text{Peat} \;...\; 4C_{20}H_{22}O_8 + 24CH_4 + 32CO_2 + 4H_2O$$

Marsh-gas. Carbonic anhydride. Water.

or more simply in Watts's Dictionary (under "Wood"),

$$\text{Wood } C_{34}H_{48}O_{22} + 3O = C_{20}H_{22}O_8 + 6CH_4 + 8CO_2 + H_2O.$$

It is a common mode of decomposition of woody fibre to give out marsh-gas; but we can hypothetically form the humic acids without this evolution, as Malaguti did by merely adding oxygen and drawing off carbonic acid; and this I conceive to be mainly the action in bogs in cold climates. The evolution of marsh-gas is found also in peat bogs; but I suppose it to be a non-essential part of the process, although sometimes this gas escapes in great abundance.

Liebig also supposed a decomposition of woody fibre by removal of carbonic acid and water only, not accounting for marsh-gas. In peat bogs we have products of decomposition removed in the form of brown matter; so far as we see, this brown matter has less oxygen than the woody fibre, judging from all the analyses, and the peat left has less oxygen; so that oxygen has been removed, but carbon also flows away in the brown matter. The removal of carbon with less oxygen than the original substance would increase the proportion of this latter element, whereas its diminution leads to the conclusion without further observation that a higher oxide of carbon escapes; this is no doubt the carbonic acid removed finally by the abundant water. The apparent deoxidation is really an oxidation. But hydrogen does not much change its proportion; this shows that some of it also is removed, since the diminution of the other two elements would otherwise increase its amount. Possibly the marsh-gas may account for it all, although I am not inclined to grant the formation of so much.

This reasoning leads us to the conclusion that the effect is due chiefly to oxidation—the splitting-up of bodies into gases, such as in fermentation is effected in warmth; if by putrefaction, then also at a higher temperature; and in a former paper I showed that it begins very slightly below 12·2° C. (54° Fahr.). I do not know if woody fibre will give off gas at a very low temperature; it probably ceases

early; nor do I know if any bodies give off marsh-gas if merely kept moist and exposed to the air, as the upper layers of peat often are with us, without being immersed in water.

However this may be, the effect of air and moisture in reducing plants to peat is rapid; and it is seen best in plants of delicate form, small plants, such as mosses. *Hypnum purum, triquetrum,* and *tamariscinum,* with a *Galium* (as named by Dr. Schunck, F.R.S.), were those which grew above the peat which was found most rapid in growth at Cairn Monearn, and of which, in all probability, the whole was composed. This will explain why the structure was so fine, and why it shrunk into such a small space and became so heavy; it did so because the feeble stems crumbled on a slight attack by the atmosphere. Had they been strong pieces of wood, they would of course have resisted; and thus we find peat of strong fibre very light and bulky. It is often supposed that wood makes peat; and we hear of forests falling and becoming peat. The forests may interrupt drainage and aid mechanically; but when large pieces of wood become entangled in mosses they endure very long. It may be that the outside or surface, after being attacked, prevents the action on the rest. We find forests preserved for ages, and real peat formed in a few years; this is enough to lead us to give up the opinion that trees form peat bogs. If any peat is formed of the wood of trees, it can only be after a very long time, when the thick stems have decomposed.

When I say that the formation of marsh-gas is not an essential to peat, so far as I know, I do not mean to deny that it does always occur, especially in warmer regions than I have studied; and indeed we know, from several sources which I have quoted, that the formation takes place; but then this is the action which allies itself to ordinary vegetable decomposition, and not to peat-making.

The inclination to break up into gaseous bodies is opposed to the general character of peat, a large portion of which is disposed to remain stable, to preserve itself little changed, and to preserve other bodies also; and thus we find so much of the matter which is compelled by the atmosphere to decay, passing off quietly in solution, innocent of all effect on the air, even the carbonic acid being soluble enough. When mosses decompose, sulphuretted hydrogen is eliminated also, as from other organisms; but whether it ever reaches the atmosphere has not been shown, so far as I know; nor do we know if it is essential. This gas is readily oxidized in abundance of water with free air.

The 'Giessen Jahresbericht' for 1864, describing a paper by J. Websky, says:—"According to him, *Sphagnum* is the only proper peat-former. By means of its peculiar cellular structure it can take up 200 times its weight of water, and live luxuriously in such waters as give very little ash; on the other hand, when it decays, it gives out the greatest part of its inorganic substances in a comparatively short time, thus enabling the plant to continue its growth." He gives analyses of three ashes, of which one peat contained 3·88 per cent., the second 5·74, and the third 2·83:—

	A.	B.	C.
Potash	13·04	0·44	1·08
Soda	6·36	0·23	1·17
Lime	10·31	4·72	5·96
Magnesia	5·15	1·15	1·57
Alumina	2·86	3·96	6·98
Peroxide of iron	14·30	3·51	4·19
Sulphuric acid	4·47	1·12	2·88
Chlorine	4·28	0·18	0·66
Silica	9·02	4·9	
Phosphoric acid	4·12	1·77	2·05
Undetermined	25·83	76·56	73·04
Lost	0·27	0·15	0·57

I cannot agree to put *Sphagnum* on such a lofty plane at present.

Resins, Fat, Waxes, &c. found in Peat.

It seemed to me at one time that the retention of hydrogen during the decomposition of woody fibre was an explanation of the existence of the large amount of bodies containing much of that element—making in this manner a theory which would suit the opinion generally held by the few who have thought of the subject.

Mulder, as has been mentioned, has paid much attention to the resins.

It is known that when peat is washed with ether or bisulphide of carbon or alcohol, benzol, or any of the liquids used for dissolving fats or resins, we obtain substances very rich in carbon and hydrogen. The origin of these compounds does not seem to be any decomposition in the dead plant, but may be traced, I think, with certainty to the living one.

I suppose now that the substance is one which, like the resins in wood, resists decomposition under water, and to a certain extent accumulates; that is, the woody fibre is washed away and the resins are left to be greater in proportion than at first.

This gives the reason why the old peat is so much richer than the new. The early stages give us it light and fibrous, and so elastic that it cannot practically be compressed for useful purposes without great expense, and the fibres must be broken so as to destroy the elasticity of the moss. Time breaks up all the fibres and gives us a substance containing few, if any, distinct forms, the spaces all filled up, and the resinous part much increased, along with the total density. The weight per cubic foot varies in my specimens from 1 to 6, *i. e.* from 0·2 to 1·2.

If the action went on very long, we should have nothing at last but the hydrocarbonaceous matter remaining: the woody fibre would all be gone; and we should have a substance such as a hydrocarbon (a resin, a wax, or a fat) by

itself. I can believe that in peat bogs this action soon ceases; that is, the separation ceases, and the resins &c. begin after a time to be washed away also. If this were not the case, we should certainly in some of the old peat deposits have some compounds richer than we have. Or it may be that at a certain time, when the peat ceases to oxidize and form soluble compounds, the permanent stage has set in, and the substance may remain unchanged for ever, as in the beds to which geologists have attributed great age. In any case some hydrocarbonaceous substance remains. This has been examined by several chemists; but speculation, as well as a little work on the subject, has given me the idea that it deserves further research. That which I have found is very solid, and some of that also which I have seen described.

The observations of Mr. Binney and many other authorities quoted make it certain that oil flows from the peat; and we have thus plants forming both liquid and solid oils, so to speak, on the surface, and giving the former at least out whenever the plant becomes so much decayed and loses so much of its substance as to be unable to hold the fluid. One feels at a point like this strongly inclined to look to this source for many of the resinous and paraffin products found under various names, earth-wax or ozokerit included; and who knows if we may not even look to this for the great supplies of American oil, and the source from which the shales were impregnated with their hydrocarbons (as intimated by Mr. Binney's speculations)? Whilst writing this there is an account of oil being found under a large heath in Germany, the "Luneberger Heide."

The resins and waxes of peat have received some attention; and most of my work is useless, as others have done the same: but there are still some questions left; and that concerning the formation of these bodies is one not quite enough considered.

The solid resin, as obtained by evaporating the solution of naphtha or ether with which the peat is washed, is hard and black, with a fracture like that of wax. When peat is utilized as a source of oil, it is as a distillate that this is obtained; but here we have a crude, solid product, exactly as it exists in the peat, and several trials gave 6 per cent.: in some cases it rose above, and in some it was below this amount; but the variations found have not been great.

This crude substance, analyzed in my laboratory, gave

Carbon	73·391	73·545
Hydrogen	10·78	10·49

The melting-point was 57° C.; after some purification, 60°.

After being kept at the boiling-point some time, during which a more volatile matter came off, analysis gave:—

Carbon	80·844	80·949
Hydrogen	12·00	12·004

If rapidly distilled, a white solid product is brought over; if slowly, an oil of a pale yellow, and a pleasant odour when the empyreuma has been driven off. When 142 grammes of the crude was distilled, the results were:—

At 100° C.	no product.
200	1·5 of an aqueous liquid.
200–280	0·5 oil.
325	boiling, a pungent smell of acetic acid.
352	30 cub. centims. of oil, and 2 of water.

The specific gravity of the oil which came over below 350° was 0·857, above that temperature 0·819.

The oils of the distillate were not analyzed separately. That may be done some day; but the amount being small, a general result was sought; and there was found

Carbon	83·86
Hydrogen	12·70

When this oil is kept for some weeks, it darkens like some resin oils; but I found one specimen which scarcely

did so in six months. There were indications, it seemed to me, that it was allied more to the paraffin oils; but I found no solid paraffin in it, and gave up the inquiry until more of the substance should be obtained.

Dr. Schorlemmer, however, who has had more experience in separating these oils, appeared to be very certain that a paraffin did exist in it. We are thus led into the belief of a great variation in the composition of this soluble part of the peat, as, indeed, the indications in the writings of former observers would show.

When one of the specimens, namely that from a heavy peat from Stornoway, was boiled with caustic soda, nearly all was dissolved, and the solution, when acidified, gave a resin closely, in all external characteristics, resembling shellac. The insoluble was a thick oleaginous body; of that, however, I had little, and so I must leave this portion for another time or for other persons. It was at first no part of my plan to examine it all, although it has now become more interesting than it was at first.

I considered a little if this could be made a manufacturing process, and made an experiment with 6 gallons of naphtha and dried peat; but I did not obtain above half the amount found on a small scale. This was owing to the difficulty of washing large quantities. It seemed to me too that it would require too much labour to boil down the naphtha, and to remove it all from the peat, which absorbs enormous quantities. These difficulties cannot be solved except by trials on a considerable scale. At the rate of 6 per cent. on the dry peat, an acre of good material ten feet deep would give 67 tons of the crude material, which, even allowing it to be worth no more than threepence a pound, would be worth about £1800 sterling. But I have no idea of the expense of obtaining this, although I think its value would be more when obtained. It might be a better mode of clearing peat-ground than some plans

adopted at present; and, at any rate, if of no use today, one may amuse oneself in thinking of this as a mode of growing oil to be employed perhaps when whales, oil-wells, and good coal and shale are rather scarcer than at present.

Peat thus becomes, to use another comparison, like an olive tree to the north, so far as the light of the oil is concerned, although the want of beauty and the want of the edible part will certainly strike one as a contrast, little as the beauty of the olive tree is.

Peat from Helensburgh.

This peat, when dried, was treated with bisulphide of carbon. The amount of fat &c. extracted is 5·55 per cent.

The bisulphide was driven off the peat, and the peat was then treated with alcohol. 2·4 per cent. of mixed resins &c. was extracted.

This substance nearly melts at about 100° C., and when cold is quite hard and brittle.

Peat from Stonorway. January 1875.

5·5 per cent. of oil and resins was extracted from this peat by warm naphtha.

2·2 per cent. was extracted with alcohol after the peat had been treated with naphtha.

7·5 per cent. oil and resins was extracted from the peat with alcohol alone.

This substance seems much harder and more resinous than that extracted with naphtha.

The peat was dried before it was treated.

Peat from Durris. February 24, 1875.

	grains.
Box and wet peat weighed	16700
Box . . .	4200
Peat . . .	12500

	grains.
Box and peat, after 4 weeks and 3 days,	7·600
Box . . .	4·200
Peat . . .	3·400

= 27 per cent of dry peat.

The amount of resin extracted by naphtha = 5·04 p. c.

This was peat of the most recent growth.

Moss from Cairn Monearn, Durris. February 24, 1875.

The moss contained 77·91 per cent. moisture.

1 lb. of the turf was dried and treated with warm naphtha; the amount of resins &c. extracted is 0·26 per cent. calculated on the wet moss. Calculated to the dry moss it is 1·17 per cent. (This moss was *Hypnum* chiefly.)

This resin or grease is very soft and has quite a different smell from that of peat.

The moss was treated with alcohol after the naphtha. A good deal of colouring-matter was extracted, but very little if any resin.

A fresh *Sphagnum molluscum* produced only 0·8 per cent. of the resins.

With these facts before me I come to the conclusion which I mentioned previously, that the highly combustible substances in peat have been formed in the growing and are left in the partly decomposed peat. It may be that they undergo a change; but some of them seem scarcely to do so. If I am right, then, the hydrocarbons and hydrocarbonaceous substances, by which I mean substances not entirely hydrocarbons, may differ in peat according to the plant which was at the origin; we may therefore receive the words wax and oil, pitch and resin as correct, and we may even suppose that the isolated masses of ozokerite, so abundant in Galicia, may owe their existence to ancient peat now destroyed. Why should it be destroyed? In circumstances favouring oxi-

dation we may suppose all the woody matter to be removed, leaving no trace of woody structure; or we may suppose that the lighter oils from the peat carried away in solution some of the heavier hydrocarbons and left them in the earth in masses, or perhaps carried them into hollows where the one evaporated and the other was left. The numerous varieties of the pitch and resin species, including elaterite, may find a similar origin.

The part soluble in ether was found much softer in the mosses tried than in the peat; there is here therefore a clear proof that some oleaginous substances were either carried off or hardened; and as it was shown that some of the substances rich in hydrogen may have been removed, we must fix on those most easily carried by water, oils and soft fatty substances. As the peat decays or diminishes in bulk, there will be less power of retaining oily matter, and it will be driven out from want of fine fibrous material to harbour it.

After concluding that the resinous or pitchy matter of the peat had grown in the plant when fresh, and was not a product of decomposition, it was an easy step to coal; whether a correct one or not is to be learnt.

In a lecture by Prof. Dawkins we find a short history of the discovery by Prof. Morris of spores and sporangia in coal, with the labours of Mr. Carruthers and Prof. Huxley upon the subject. He says, "There can be no doubt that the bituminous matter of coal is derived nearly altogether from the spores and sporangia of fossil vegetables allied to the club-moss. And if we come to examine what the character of the club-moss is, we can easily understand how the bituminous material in coal is derived from like objects . . . . Our bituminous coal derives its bitumen from this altered resinous matter, which was first of all stored up in fruits of that kind, and afterwards altered by the subterranean heat into bitumen, thus form-

ing our bituminous or blazing coals." He then compares the recent club-moss (the *Lycopodium*) and the ancient one (the *Lepidodendron*), the latter being seen as a specimen at Newcastle 3 feet in diameter and 49 feet high, whilst the "living club-mosses are not much larger than your finger, and the largest of them is not more than six feet high." Prof. Huxley says, in a lecture on the formation of coal, "If, as I believe, it can be demonstrated that ordinary coal is nothing but 'saccular' coal which has undergone a certain amount of that alteration which, if continued, would convert it into anthracite, then the conclusion is obvious that the great mass of the coal we burn is the result of the accumulation of spores and spore-cases of plants, other parts of which have furnished the carbonized stems and the mineral charcoal, or have left their impressions on the surfaces of the layer." "These facts do not permit us to suppose that coal is an accumulation of peaty matter, as some have held."

Dr. Dawson, in a paper "On Spore-cases in Coal," says "that sporangite beds are exceptional among coals, and that cortical and woody matters are the most abundant ingredients in all the ordinary kinds; and to this I cannot think that the coals of England constitute an exception."

I have not examined coal; but judging from the peat, it seems to me probable that the truth may lie near the above. The resins of trees are not found only in spores; and it may be that the bituminous or, rather, the hydrocarbonaceous parts of coal were formed from the parts of the plants containing the resins or similar bodies, no matter what we call them, fats and waxes included. This, I am disposed to think, is the method with peat; that it should be so with coal seems most natural.

If so, it may be well to review the statements as to the length of time needful to grow a foot of coal. But the actual measurement is impossible. If we compare the

mosses of the present with the *Lepidodendra* of the past we find a capacity of growth which must compel us to be careful in our calculations. The rapidity may have been as much greater as the bulk of the tree is greater than now; we see the same rapidity and bulk in warmer climates. If we calculate only ten feet in a century, we obtain two of dry peat, and at least one of very hard peat with our present growth; with the tropical growth there may be ten times as much. If the growth is in water, the speed seems to be much greater, and gradually ceases as it rises above the level. This would favour the idea so consistently advocated by Mr. Binney, and still not render comparatively dry-grown coal impossible.

I have tried to make the calculation as to peat—that is, to find by the amount of the resins in the peat and the plant the length of time required for the change; but it cannot be done until we know better the actual decomposition. This will be an interesting inquiry; but it must be understood for every class of plants forming peat; so far as I know, the proximate analysis of many mosses does not exist in chemical books.

Peat must henceforth be classified with much more minuteness than hitherto; and the best mode must be found.

This must not be forgotten—namely, the examination of the plants for resins, fats, and allied bodies; and this, if I continue the subject, will be considered a prominent point. It will also be important to find in what part of the plant these substances exist; for although in some the larger portion be confined to the spores, it does not seem to be well to rest entirely on this belief.

In other words, I should conclude that the plants of coal as of peat would to a great extent, and sometimes wholly, decay, leaving a fine material containing the unoxidized and insoluble matter, all or nearly all of the fats,

resins, and bodies containing much hydrogen, no matter in what part of the tree they existed.

Peat as a Water-reservoir.

This point is proved: peat is a great reservoir; and its removal is a great loss of water to the lands below in rainless weather. It is not proved that the existence of wet peat causes an increase in the fall of rain; but it may possibly be the case; it is, however, known that it prevents the showers of the uplands from falling down the streams as sudden floods.

We owe much in this county to our abundant rainfall, and still more to its very general distribution throughout the year; but it may not be well known that in Lancashire the want of water is becoming yearly more felt, and the showers, however frequent, are quite insufficient. It is not that the amount which falls is not enough, it is that we have no means of preserving that which does fall. We have invented no way of keeping it but fearfully expensive reservoirs difficult to maintain; and we neglect the simple and inexpensive means which are put in our way. Peat-water may be readily obtained. It may not for all purposes be the best unfiltered; but when filtered it is seldom that it can be objected to. We live as in the days of agricultural innocence, and expect that the water-supply and the sewer shall be one for a district. The filtering of peat-water through the soil below it, or through the lands on a lower level is a common act of nature, and to a certain extent, can be imitated; and the objectionable character which even a slight amount of peat-water sometimes has may in this way be obviated. By destroying peat-bogs on hills we destroy valuable reservoirs made cheaply and maintained cheaply—that is at the price of the rent of the land at most, but not always at this price. If we destroy them and make reservoirs below the level of

the surface, we know something of the danger as well as expense. A peat-bog 10 feet deep will hold as much water as a reservoir of the same dimensions $7\frac{1}{2}$ feet deep; it requires no digging, and may be used for shooting over and for health. It retains the water better in summer than our reservoirs; it is in fact a covered reservoir, and would make a valuable covering to those hills which have works below continually demanding fresh streams.

One of my proposals therefore is to grow our water-reservoirs instead of digging them.

Drying up Swamps.

If peat can be so readily planted and grown so rapidly, we have before us an entirely new field of industry; we can make dry land out of swamps. These places are numerous, and are in almost all cases absolutely useless, and in most injurious. We may be able to set in motion a peat-growth which will in a very moderate time make such an amount of dry material that the water-level will be far out of ready reach. Indeed we do see this done constantly, as the extracts given here show; and we have only to carry out the idea further, and, instead of making dykes to keep out the water of large portions of, say, Holland or of our own fenny districts, set in motion a peat-growth which would in many places be useful. The cases in which this is possible are, to my mind, very numerous; and the expense would not be equal to the maintenance of pumps. The plan would raise the level of already enclosed fields, and growth might be allowed to proceed till the exact amount of moisture required was obtained. The next question, of course, that will be asked is, What would be the value of such land? That would vary; there are cases where it would be a great benefit merely to have something as solid as peat to walk upon. Of course we must remember that this cannot be done over

a great district where the land is already in use, simply because its value would be destroyed for some time; but it could be done wherever there is fresh water and not too much heat. It is well known that vegetation is an admirable protection against the encroachments of water; but peat would not suit water in rapid motion.

However contradictory it may appear, I believe that the growth of peat-bogs is a most valuable method of obtaining solid dry ground in many cases; and later I purpose showing how far it has succeeded unintentionally. Indeed I may say I have myself seen it done in Scotland by force of nature and neglect.

Heating-power.

In calculating the heating-power, it is not easy to come to practical results when comparing different substances; but we can compare substances of the same character well. The trials made by Mr. Alfred Fryer in my laboratory gave Wigan coal as 14000, peat 5100 and 4850, oak 9700, and deal 9300. If, then, we say that peat gives half the heat of coal, we must mean very inferior coal or very excellent peat. To say one third would be safe in a general way, unless we knew distinctly the character of the peat and coal to which we were specially alluding; but with Prof. Gysser I shall say 0·4, as he certainly has examined many specimens.

Gysser's Table of Heat produced by Wood and Peat.

(From Professor Johnson's 'Peat and its Uses,' p. 97.)

	°
Beech wood split, air-dried	1·0
Peat condensed by Weber and Gysser's method, air-dried, with 25 per cent. moisture	1·0
Peat condensed by Weber and Gysser's method, hot-dried, with 10 per cent. moisture	1·48
Peat charcoal, from condensed peat	1·73
Same simply cut and air-dried	0·80
Beech charcoal	1·90

	°
Summer oak wood	1·18
Birch wood	0·95
White pine wood	0·72
Alder	0·65
Linden	0·61
Red pine	0·61
Poplar	0·50

Karmarsch's Heating-power by bulk (p. 96).

cub. ft.			cub. ft.	
100 of turfy peat, on the average =			33	of pine-wood in stubs.
„	fibrous	„	90	„
„	earthy	„	145	„
„	pitchy	„	184	„

Waste space in peat and wood as usually piled not far from 40 per cent.

Karmarsch's Absolute Heating effect (p. 96).

100 lbs. of turfy air-dried peat, on the average =			95	lbs. of pine wood.
„	fibrous	„	108	„
„	earthy	„	104	„
„	pitchy	„	111	„

According to Brix the weight per cord and relative heating effect of several air-dried peats, the heating power of an equal bulk of oak wood being taken at 100 as a standard, are as follows, *bulk for bulk*:—

	Weight per cord. lbs.	Heating effect.
Oak wood	4150	100
Peat from Linum, first quality, dense and pitchy	3400	70
Peat from Linum, second quality, fibrous	2900	55
„ third quality, turfy	2270	53
Peat from Büchsenfeld, first quality, pitchy, hard, and heavy	3400	71
Peat from Büchsenfeld, second quality	2730	64

Sanitary Effect.

One of the most important purposes for which the growth of peat may be used is the conversion of the un-

wholesome class of swamps and marshes into sanitary agents. It is pretty well known that decomposing matter gives out gases and emanations which produce many forms of disease; but I do not know if it can be proved that malaria, which produces its peculiar class of fever and of ague, can be said to arise entirely from a decomposing body or the products of decomposition; the evidence points also to living plants and to substances arising from them. I will not attend to this question at present, because in either case we find that the peat-bog has the advantage. If it be the living plant, we find no malaria, no fever, no ague caused by peat; if it is the peculiar class of plants, mosses, sphagnums, heaths, or whatever else grows there, which causes freedom from these evils, then let us cultivate that plant. On this point Macculloch says that malaria "is notoriously not produced by dead peaty bogs or by peat which carries no vegetation" (p. 61, 'On Malaria,' by John Macculloch, M.D., F.R.S., 1827). Immediately after, however, he says that it would be easy to show that peat lands are not exempt from malaria when there is vegetation enough; and he attributes the absence of malaria on growing peat-bogs to the cold. If, however, we look at the limits of malaria, we find it about St. Petersburg with a mean temperature of 3°·38 R., or 39°·6 F. (from Mühry's 'Klimatologische Untersuchungen,' 1858). In Oesterlen's 'Handbuch der Hygiene,' we have the limit put as 5° C., or 41° F. (edition of 1857), which is considerably lower than the mean temperature of Edinburgh and Dublin.

Macculloch supports me so far in saying that gases do not come from formed peat; he is inclined to think that the plants may produce malaria in decomposing. The temperature may influence the character of the plant, also the character and speed of its decomposition; but when the plant is transformed into peat, the great fact that the

main quantity of matter comes off in water and in a visible form, is one that must lead us to an explanation of the rare presence if not entire absence of intermittent or other fevers as a result of living near peat. The whole amount of matter removed by decay is much less than with other vegetation, since the bulk is continually accumulating instead of dispersing into the air to do mischief.

The subject may be dark; but we must take the facts in a broad way until we can narrow them; and if peat-bogs produced malaria at a temperature at which we know it to be producible, Ireland and Scotland would not be places of resort for health. If, therefore, peat-plants can be made to grow and push out the unhealthy, or to cover over wet surfaces which have been unhealthy, a great point is attained.

The water from the bogs has powerful antiseptic qualities, as we know; and that these qualities should be associated with the usual emanations from putrefying vegetable substances is a contradiction which is not to be thought of; neither can we believe with some that common putrefaction goes on in peat-bogs as in marshes, so long as the water has such different properties. The assertion, however, that decomposition giving out gases does take place demands attention; I suppose that in warmer climates than ours it does not arrive at its conclusion so soon as with us. This is somewhat of a repetition; but this point rises again here naturally; besides, I am not attempting to decide, but am trying to reconcile facts, or such statements as I believe to be well grounded.

If, then, it is the *colytic* action of the peat, the action restraining decomposition, to which the peat-bogs owe their healthful atmosphere, the plant may be of little consequence. I have not beside me a list of all the plants found in marshes; but I believe they are always different from those in peat-bogs, and take it so for granted at

present. The plants, then, may be the cause of the peculiar mode of decomposing which produces the *colytic* liquid. There are some who tell us that it is the heat only; but again we are told that peat may be found in all countries in Europe. Dr. Anderson even says that he has obtained real peat from the island of Sumatra (see Steele on 'Peat Moss or Turf Bog,' p. 342). He does not tell the height; the highest point of the Indrapura chain is a little above 12000 feet (12140). This at least shows that latitude has nothing to do with the question.

I do not know the height at which the peat grows in the southern regions of Europe, and therefore cannot speak with that certainty which I desire; and I may not be able to find it; I therefore call attention to this point for sanitary purposes; and I trust the question will be settled by the attention of many, how far peat-growing can be introduced for the destruction and drying-up of swamps. Those who make the trials must take care that the peat-producing plants are not overwhelmed by an excess of other plants, or the water from other plants, as that may prove their destruction.

The *Eucalyptus* is being introduced into Southern Europe and into North Africa for the drying-up of swamps and the destruction of malaria; and it is apparently most efficacious; it cannot, however, be used for increasing the height of the solid matter, although when the marsh is not deep this tree is said to dry it up rapidly. This, however, is somewhat different from raising the level of the ground to be trodden; and it cannot entirely take the place of peat. I do not know how far north it may succeed. It seems to give out at all times an odour of its peculiar oil, and is therefore a medicinal tree. The peat acts by preventing emanations; and prevention is proverbially better than cure.

If it should be found that warm climates, or climates

such as Italy, should be unfitted for the growth of peat on the swampy ground, it is otherwise on some of the hills; and it may in some parts be convenient to grow peat-bogs above and lead the disinfecting or colytic water down to the land which is malarious, thus operating as a sanitary and agricultural measure on the lower ground, and as a source of water and fuel on the higher grounds. This is, of course, only an idea; but it is not one to be forgotten.

Dr. Morgan, in the British and Foreign Chirurgical Review, vol. 26 (1860), p. 483, has an article on Phthisis in the Hebrides. He there shows, by evidence of a very extensive character, that the absence of that disease in the islands and western coast seems to be owing to the use of peat and the custom of the inhabitants to live constantly in the smoke. He quotes Dr. Anderson, of Tyree, who says, "I have not met with one case where the smoke was allowed to find its way through the roof." Dr. Mathieson, in Skye, had met with only two cases where there was no chimney, to eighteen where there was a chimney. The disease also, when it does come, appears to be more prolonged than usual. The whole article of Dr. Morgan's is very interesting; and it shows how much may be done by thus disinfecting the air, even in cases where cleanliness of persons and houses is not specially attended to.

Much of this communication consists simply of notes to be elaborated afterwards; additions must also follow in a Second Part.

XXXIX. *On a Colorimetric Method of Determining Iron in Waters.* By Thomas Carnelley, B.Sc. Communicated by Professor H. E. Roscoe, F.R.S.

Read October 20th, 1874.

Of late years the analysis of water has become of such importance that any improvement in the methods employed in that analysis will, it is thought, be acceptable, however small such an improvement may be; and it is with this consideration that the following paper is submitted to the Society.

In the determination of heavy metals in water, with the exception of lead, great inconvenience arises from the want of rapid and accurate methods of estimating very small quantities; and it is to remedy this inconvenience in the case of iron that the following method is proposed. Besides accuracy it fulfils both the other requisites, viz. rapidity and the power of determining exceedingly small quantities; for without any evaporation 1 part of iron in 13,000,000 parts of water can be detected, and a determination made in less than fifteen minutes, the smallest amount of ammonia which can be detected by the well-known Nessler test, without concentration, being only 1 part of ammonia in 20,000,000 parts of water; and moreover, as water will admit of evaporation without loss of any iron it may contain, the iron which can be estimated may be reduced to almost an infinitely small quantity.

The method consists in the comparison of the blue colours produced by adding to a solution of potassium ferrocyanide, first, a solution of iron of known strength, and, secondly, the water in which the iron is to be determined.

The standard solutions and materials required are as follows:—

'1) *Standard Iron Solution.* This is prepared by weighing out 0·7 grm. of ammonio-ferrous sulphate (=0·1 grm. Fe), dissolving in water, and adding 1 cub. cent. of the sulphuric acid; the iron is next oxidized by adding an exact sufficiency of the potassium-permanganate solution from a burette, and the whole diluted to 1 litre. Of this solution 1 cub. cent. =0·0001 grm. Fe.

(2) *Solution of Potassium Permanganate.* This must be moderately dilute; but it is not necessary that it should be of standard strength.

(3) *Standard Nitric Acid.* Is prepared by diluting 50 cub. cent. of pure strong nitric acid to 1 litre.

(4) *Potassium-Ferrocyanide Solution.* Is obtained by dissolving 1 part of the salt in 25 parts of water.

(5) *Strong Sulphuric Acid* diluted with an equal volume of water.

(6) *Two similar glass cylinders and a glass rod.* The former should hold rather more than 200 cub. cent. each, the point equivalent to that measure being marked on the glass.

(7) *A burette* marked to 0·1 cub. cent. for the iron solution, and an ordinary burette for the permanganate.

(8) *Three* 1 *cub.-centim. pipettes* for the ferrocyanide, nitric acid, and sulphuric acid respectively, the one for the last being marked also to deliver 0·5 cub. cent.

The following is the method of analysis employed:—

A measured quantity of the water less than 1 litre in bulk is taken, the amount being regulated according to the quantity of iron contained in the water, which is judged by a previously made qualitative experiment of adding 1 cub. cent. of the ferrocyanide to a portion of the oxidized water. One cub. centim. of the sulphuric acid is added, and then the permanganate from a burette till a

permanent faint pink colour is obtained; the whole is made up to 1 litre, when it forms what may be called the "water-test solution." In each of the cylinders 1 cub. cent. of the potassium ferrocyanide is added, and then a measured quantity of the water-test solution put into one of them (x); both are next filled with water up to the mark, and 1 cub. cent. of the standard nitric acid added to each. After x has been well stirred, the standard iron solution is gradually run into y, the liquid being stirred after each addition, and the colours in the two cylinders compared by placing them side by side over a sheet of white paper in front of a window; this is repeated till the colours in each of the cylinders appear to be equal, which point completes the operation.

Every cubic centimètre of iron solution used corresponds to 0·1 milligrm. of iron, from which the amount of iron added to cylinder y can be calculated. Then, assuming that equal shades of colour are, *ceteris paribus*, produced by equal weights of iron, the amount of the latter in cylinder x is equal to that added to y; and since the volume of the original sample of water in the test solution is known, and also the volume of the latter put into x, the amount of iron in a measured quantity of water can therefore be calculated. The volume of the test solution put into cylinder x should be such as not to require more than 5 cub. cent. of the iron solution to be added to y to produce an equal shade; for if more be added the colour obtained will be too dark to compare with ease and accuracy.

If the sample of water contains such a small amount of iron as, after oxidation, not to give a coloration directly with the ferrocyanide and nitric acid, a sufficient quantity of it must be evaporated with half a cubic centimètre of the sulphuric acid till it occupies from 100 to 200 cub. centims. The liquid is then poured into a flask, together

with the rinsings, and oxidized with permanganate to a very slight excess, and then filtered, so as to separate any precipitate, and also to remove the excess of permanganate. The fluid thus obtained is next tested as before by adding the whole or a known part of it to one of the cylinders containing the ferrocyanide. When the water, after being filtered, has still a cloudy appearance, as is the case with sewage and polluted rivers &c., a known quantity of the filtered water must be evaporated to dryness and ignited, the residue dissolved in a small quantity of hydrochloric acid and filtered, washed with water, and the free acid in the filtrate as nearly as possible neutralized with ammonia, and then 1 cub. cent. of sulphuric acid added, after which it must be oxidized with permanganate, then filtered, if requisite, to remove excess of permanganate, and the iron estimated as before. A green colour may be sometimes obtained, instead of the pure blue; this is owing to a slight quantity of unreduced permanganate being present, which, however, is of no consequence, as, with a little practice, the green tint may be compared with the blue, and correct results obtained; still the comparison may be rendered easier by adding 1 to 2 drops of permanganate to the cylinder into which the standard iron is run, and which by this means will also assume a green tint. Experiments were made with reference to this point; and it was found that the presence of not more than a few drops of unreduced permanganate has little or no effect on the results obtained, the only consequence being the change of tint, but not of depth of colour.

Potassium permanganate is employed as the oxidizer, instead of nitric acid, because (1) the oxidation is performed much quicker than it would be if nitric acid were used, as in the latter case the liquid would have to be heated; (2) it can be added to exactly the right point, which could not easily be done with nitric acid; (3) an

excess of the latter is very detrimental to the accuracy of the method; for from experiments made in relation to this point it was found that when the amount of free acid present in 200 cub. cent. is more than 0·0025 cub. cent. of the strong acid, it renders the colour deeper than it otherwise would be.

One cubic centimètre of the standard nitric acid is added to each of the cylinders, (1) because it renders the reaction much more delicate; (2) because the colours produced in the presence of this amount of free acid are almost always of the same tint, being of a pure blue, whilst, when no free acid is present the colour varies, even when apparently of the same depth, from a blue to a bluish green, which renders them less easy to compare; (3) because it destroys the effect which the presence of a small quantity of any free acid previously existing in the liquid might have in altering the shade of colour produced; for, with reference to this point also, from a series of experiments made it was found that when the amount of the free acid present, in addition to the 1 cub. cent. of standard nitric acid added, is only small, *i. e.* less than 0·05 cub. cent. of the strong acid in 200 cub. cent. of water, it has no effect on the depth of colour produced. When any free acid exists in the water to be examined, it must, before being oxidized, be made as nearly neutral as possible with ammonia, and the iron then determined.

In order to test the method as a whole, the experiments given below were made, and were carried out as follows:—

A known weight of ammonio-ferrous sulphate was taken, dissolved in water, 1 cub. cent. of the sulphuric acid added, and then oxidized with permanganate, and the whole made up to 1 litre. The iron in the solution thus obtained, was determined as described above. One of a number of series of experiments thus made is given below.

Weight of ammonio-ferrous sulphate taken = 58·00 mgr.
= 8·28 of iron.

After oxidation made up to 1 litre;

∴ 1 cub. cent. = 0·00828 milligr. Fe.

(*a*) (1) 50 c. c. of this solution taken, = 0·414 mgr.
4·1 of iron solution required, = 0·410 } Fe.
(2) 4·2 „ „ „ = 0·420

As a mean of these two experiments, it is found that

	mgr.
the solution contained	8·30 Fe.
Calculated	8·28 „
Difference =	+0·02

(*b*) 500 cub. cent. (=4·142 milligr. Fe) of the original solution taken, and diluted to one litre.

∴ 1 cub. cent. = 0·00414 milligr. Fe.

(1) 100 c. c. of this solution taken, = 0·414 mgr.
4·2 required, = 0·420 } Fe.
(2) 50 of this solution taken, = 0·207
2·1 required, = 0·210

	mgr.
As a mean, the solution contained	4·20 Fe.
Calculated	4·14 „
Difference =	+0·06

(*c*) 225 cub. cent. (=1·863 milligr. Fe) of the original solution taken and diluted to 1 litre.

∴ 1 cub. cent. = 0·001863 milligr. Fe.

100 c. c. of this solution taken, = 0·1863 mgr.
1·8 required, = 0·1800 } Fe.

	mgr.
∴ the solution contained	1·800 Fe.
Calculated	1·863 „
Difference =	−0·063

(*d*) 120 cub. cent. (=0·99 milligr. Fe) of the original solution taken and diluted to 1 litre.

∴ 1 cub. cent. = 0·00099 milligr. Fe.

c. c.		mgr.	
200 of this solution taken,	=	0·198	Fe.
1·8 required,	=	0·180	
∴ the solution contained	.	0·900	Fe.
Calculated		0·990	,,
Difference	=	−0·090	

A large number of experiments were made in this way, the results of which are given on p. 353.

In order also to test the effect which the presence of different salts has on this method, four series of experiments were made by adding known weights of the following salts to 1 litre of the ammonio-ferrous sulphate solution.

(A) Calcium sulphate, magnesium sulphate, ammonium chloride, sodium chloride, and potassium carbonate, in all 1·6 grm.

(B) Ditto, in all 0·9 grm.

(C) Magnesium sulphate, ammonium chloride, potassium carbonate, sodium chloride, and calcium chloride, in all 0·8 grm.

(D) Potassium nitrate 0·4 grm.

The solutions thus obtained were oxidized with permanganate and sulphuric acid, diluted to one litre, and the iron estimated as previously described. The results obtained are given below, the letters attached denoting to which of the preceding series they severally belong; and from them it will be seen that the presence of these salts has little or no effect.

It was also found that neutral organic matter is not detrimental to the method.

Iron found. milligr.		Iron calculated. milligr.
510·00	.....	517·00
400·00	With Salts (A).	393·40
390·00	.....	389·70
184·99	.....	178·10
90·00	.....	90·83
66·40	.....	67·80
29·40	With Salts (C).	28·70
27·00	.....	31·07
25·79	.....	25·85
17·99	.....	19·48
11·25	.....	12·92
11·70	With Salts (B).	10·86
10·10	.....	10·21
8·30	.....	8·29
6·60	.....	6·00
4·20	.....	4·14
3·30	.....	3·06
1·80	.....	1·86
·90	.....	·99
·70	.....	·68
·64	.....	·62
·62	.....	·62
·61	With Salts (C).	·57
·51	.....	·54
·51	.....	·52
·52	.....	·51
·55	.....	·53
·54	.....	·55
·48	With Salts (B).	·43
·45	.....	·45
·40	.....	·41
·40	With KNO_3 (D).	·40
·40	.....	·39
·38	.....	·39
·34	.....	·34
·33	.....	·30
·32	.....	·31
·28	.....	·31
·29	With Salts (C).	·29
·23	.....	·26
·21	.....	·23
·23	With Salts (B).	·22
·22	.....	·25
·21	.....	·21
·20	.....	·21

Iron found. milligr.		Iron calculated. milligr.
·18	.....	·20
·18	.....	·19
·14	.....	·16
·14	.....	·14
·12	.....	·10
·070	.....	·078
·053	.....	·062
·025	.....	·031

With reference to the delicacy of the method, it was found, as a mean of seven experiments, that 0·0055 milligr. of iron gave a very distinct colour on the surface, and that 0·015 milligr. give a blue colour on being stirred with 200 cub. cent. of water, and therefore that 1 part of iron produces a blue coloration in 13,000,000 parts of water containing ferrocyanide of potassium and nitric acid. According to Hartig*, however, 1 part of iron (in the form of sulphate) only produces a colour in 600,000 parts of water containing ferrocyanide; the difference of these results is due to the effect which the presence of the small quantity of free nitric acid, added in the new method, has in increasing the delicacy of the reaction.

As to the smallest differences of reading which can be detected, it was found that when any quantity of iron solution below 1 cub. cent. had been added, a difference of 0·05 cub. cent. can be discriminated; above 1 and below 2 cub. cent. a difference of 0·1 cub. cent.; above 2 and below 4 cub. cent. a difference of 0·2 cub. cent.; and above 4 and below 5 cub. cent. a difference of 0·3 cub. cent.

The following are a few samples of different waters in which the iron has been determined as described above.

* Journ. Pr. Chem. xxii. 51.

Parts per million.

Date.	Name of Water.	Amount used in analysis.	By new method.	By MnO_4.	As found by other observers.	
1874.						
Feb. 14.	Manchester Water-supply.	5 litres.	·21	.....	·287	R. A. Smith, 1864.
Mar. 20.	,, ,,	1 litre.	·18			
,, 20.	,, ,,	2 ,,	·175			
July 15.	,, ,,	1 ,,	·10			
,, 15.	,, ,,	1 ,,	·11			
Aug. 31.	,, ,,	1 ,,	·27			
Sept. 1.	,, ,,	1 ,,	·18			
,, 2.	,, ,,	1 ,,	·26			
,, 2.	,, ,,	1 ,,	·27			
,, 3.	,, ,,	½ ,,	·30			
,, 3.	,, ,,	1 ,,	·32			
,, 4.	,, ,,	½ ,,	·38			
,, 4.	,, ,,	1 ,,	·36			
,, 5.	,, ,,	½ ,,	·42			
,, 5.	,, ,,	1 ,,	·41			
Feb. 19.	Medicinal spring, Trefriw, North Wales.	30 c. c.	1600·00	1575·50 *		
Feb.	Chloride-of-iron Spa, Harrogate.	50 c. c.	290·00	290·34	289·40	Hayton Davies, Feb. 1872.
,, 16.	River Irwell, near Pomona Gardens.	¾ litre.	·86			
,, 18.	River Mersey, Northenden.	1½ ,,	·48			
,, 23.	River Dane, above Congleton.	1 ,,	·57			
,, 23.	River Dane, below Congleton.	¾ ,,	·44			
,, 23.	Liverpool Water-works, Compensation water, White Coppice, Heapy.	2 litres.	·42			
Jan. 3.	Barnsley Water-supply.	2½ ,,	·25			
Feb.	River Thames, London Bridge.	1 litre.	·19			
Jan. 3.	Cockerham Well, Barnsley.	1 ,,	·075			
Feb.	New-River Company.	1 ,,	·050		Trace.	
,,	Lambeth Company.	1 ,,	·044	.....	12·1 (Al_2O_3 & Fe_2O_3)	Graham, Miller, and Hofmann.
,,	East-London Company.	1 ,,	·038	.....	6·8 (Al_2O_3 & Fe_2O_3)	Graham, Miller, and Hofmann.
Jan. 3.	Friar's Well, Barnsley.	1¾ ,,	·005			

* The author hopes shortly to bring an analysis of this water before the Society.

XL. *Analysis of one of the Trefriw Mineral Waters.* By Thomas Carnelley, B.Sc. Communicated by Professor H. E. Roscoe, F.R.S. &c.

Read December 1st, 1874.

An analysis of this strongly ferruginous mineral water has not, so far as the author has been able to learn, been published before any scientific society; and though two general analyses of it have previously been made, the first by D. Waldie, Esq., in 1844, and the second by Dr. Hassall in 1871, and published in the form of pamphlets for public reading by Dr. Roberts and Dr. Hayward respectively, yet, as it is peculiar for the extremely large quantity of iron and alumina that it contains, and as its composition has varied considerably since it was analyzed by the last-named chemist (whose results also varied from those of the first), it is thought that another and more complete analysis will not be out of place.

The village of Trefriw is situated on the left bank of the Conway, about $2\frac{1}{2}$ miles from Llanrwst and between the latter place and Conway. The springs, which now belong to a company and are often visited by invalids, as they are said to be good for the cure of diseases of the digestive organs and of the skin, are close to the highroad which runs between Conway and Llanrwst, and are rather over a mile from the village. The entrance to them is a short way up the side of the mountain called the Alt cae Coch, and consists of an underground passage cut in the rock. There are at present two springs (formerly there were three)—one opposite and close to the entrance, the other at the end of a gallery 10 or 12 yards long to the right. The former water is used to supply the baths, and the latter exclusively

for drinking. They differ considerably in the relative proportions of their mineral constituents; but it is only the last named which is the subject of this paper.

The water, which flows into a basin cut in the rock, is said to be uniform in quantity, and issues at the rate of about 40 gallons per hour. It is quite cold, its temperature varying only within very narrow limits. As it occurs in the spring it is perfectly clear, bright, and colourless; but after a short exposure to the air it turns yellow, and deposits flakes of ferric oxide. It has no smell, but possesses a strong aud very disagreeable inky taste. On being shaken up in a closed bottle no disengagement of gas takes place. It has a strongly acid reaction, and contains neither free carbonic acid, carbonates, nor sulphides; and when first taken from the spring it is perfectly free from ferric salts.

Temperature of the external air	15·5 C.
,, ,, air at the spring	12·5
,, ,, water	11·0

	I.	II.
Specific gravity at 17° C.	1·00716	1·00570
	parts per 1,000,000.	parts per 1,000,000.
Loss on ignition	7217·5	
Precipitate formed on boiling 1 hour	32·8	
Iron	1507·0	2009·4
Aluminium	233·3	112·4
Calcium	271·3	116·5
Magnesium	134·1	45·4
Potassium	31·5	
Sodium	25·1	15·3
Manganese	trace	trace
Lead	0·86	
Ammonium (NH_4)	1·63	
Albumenoid ammonia	0·34	
Silica (SiO_2)	157·0	149·0
Sulphuric acid (SO_4)	4985·3	4512·0
Chlorine	11·8	10·9
Nitric acid (NO_3)	9·1	
Phosphoric acid (PO_4)	2·45	
Total solid contents	7370·78	6970·9
The residue dried at 310° C.	7370·00	

The preceding is (I.) the analysis made of the water collected by the author on September 8th, 1874, together with that (II.) made by Dr. Hassall in the early part of September 1871*, or just three years previously.

The following table represents the above in combination:

	I.	II.
Ferrous sulphate	4090·4	5454·3
Aluminium sulphate	1358·9	700·7
Calcium sulphate	922·3	376·0
Magnesium sulphate	670·3	225·7
Potassium sulphate	70·3	
Sodium sulphate	49·9	47·0
Lead sulphate	1·25	
Calcium chloride	.....	16·8
Sodium chloride	19·4	
Sodium nitrate	4·8	
Ammonium nitrate	7·2	
Aluminium phosphate	3·2	
Manganese	trace	trace
Silica	157·0	149·0
Albumenoid ammonia	0·34	
Bases for which there is not sufficient acid	15·5	1·4 loss.
	7370·79	6970·9

With reference to this analysis the following observations are to be made:—

(1) The determination of the total residue was first made at 180° C. as recommended by Fresenius†; and the result obtained corresponded to 800 parts per million; it was found, however, that this was much too high, the reason being that ferrous sulphate, though it loses six molecules of water at 114° C., yet retains the seventh even at 280‡. In order to drive off this remaining molecule, the residue from 100 cub. cent. of water was heated in an air bath to 300°–310° and weighed; after repeated heating, two successive weighings did not differ by more than a milligramme. In heating to so high a temperature, how-

* See 'Guide to Trefriw and Vale of Conway Spa,' by Dr. J. W. Hayward, M.D., M.R.C.S. Second edition.

† Fresenius, 'Quantitative Analysis,' 4th edition, p. 560.

‡ Watts, 'Dictionary of Chemistry,' vol. v. p. 597.

ever, there is a danger of a little sulphuric acid volatilizing by decomposition of the sulphate of iron; but by careful heating this may be avoided. A loss of ammonia will nevertheless have been incurred; but as this, together with the trace of organic matter, did not amount to more than 8 to 10 parts per million, it was not of very much consequence.

(2) It will be seen from the Table showing the supposed combination of the salts, that the total bases formed were rather more than sufficient to combine with the acids; and the base which is given above as uncombined is alumina, as it is thought that the quantity of this body obtained was rather too high; for, in addition to the total bases being too large for the total acids, the sum of the oxides ($Fe_2O_3 + Al_2O_3 + P_2O_5$) calculated from the Fe, Al, and P_2O_5, each estimated directly, is rather greater than the result obtained by weighing the three oxides together, the numbers being 2592 and 2570 respectively—difference 22.

(3) In the determination of the alumina it was separated from the iron by means of tartaric acid and sulphide of ammonium, and weighed as $Al_2O_3 + P_2O_5$; the difference between this and the determined amount of P_2O_5 gave the quantity of alumina.

(4) The phosphoric acid was estimated by precipitating with ammonium molybdate; and as the amount was only small, by weighing the precipitate obtained on a constant filter, the calculation was then made from the composition of the precipitate, which contains, according to various authorities, 3·142 per cent. P_2O_5.

(5) The iron was determined directly at the spring with potassium permanganate, and afterwards gravimetrically in the laboratory. The results obtained agreed very nearly.

(6) Several determinations were made of the alkalies; but only rather varying results, comparatively, could be obtained for the sodium. The above is the mean of four, of which the highest was 32 and the lowest 22 parts per

1,000,000, the reason being that the quantity of sodium present was only very small, so that the traces of it also contained in the reagents had an appreciable effect, though they were as pure as could be obtained. The results got for the potassium, however, agreed very nearly.

(7) The lead was determined by the method given in Wanklyn & Chapman's 'Water Analysis,' as were also the ammonium, albumenoid ammonia, and nitric acid.

From a comparison of the above two analyses it is evident that, between September 1871 and September 1874, the composition of the water has varied considerably; and though the author has not had an opportunity of seeing the analysis made in 1844 by Waldie, yet from Dr. Hassall's report, given in the above-mentioned pamphlet, it would seem that the results there given also vary much from those obtained by Waldie. The quantity of iron appears to have greatly diminished, while, with the exception of SiO_2 and chlorine, that of the other constituents occurring in larger quantities has considerably increased. A determination of the iron, made last February, gave 1575·4 parts per 1,000,000, though in this case the determination was not made till after the water had been collected some days. From this it would seem that the iron is gradually diminishing in quantity. The result, however, obtained by Waldie is very nearly the same as that got by Hassall.

From the analysis it will be seen that the Trefriw water is peculiar, as already mentioned, on account of the large quantity of sulphate of iron which it holds in solution; there being, so far as the author has been able to learn, no spring in the United Kingdom, and perhaps not even on the Continent, which contains it in any thing approaching to the same amount; while there are only a few springs known which contain it even in a notable quantity, the analyses of which have been described. The water is also

remarkable for the large quantity of sulphate of alumina and silicic acid which are dissolved in it, while the amounts of phosphoric and nitric acids, though only small, are rather large compared with what is found in most other mineral waters; on the other hand the proportion of chlorine is only small.

The other Trefriw mineral spring was not analyzed; but, from Dr. Hassall's analysis of the two waters, it appears to contain less iron and alumina, but a larger quantity of alkalies and alkaline earths than the one which is the subject of this memoir.

With regard to the geological position of Trefriw, and the source of the mineral impregnation of the springs, it may be observed that the mountains at the base of which the wells are situated consist chiefly of beds of limestone, ironstone, alum-slate, and iron-pyrites, together with varying proportions of silicates, very much fractured and dislocated, forming the northern extremity of the Bala or Caradoc beds. Up in the mountains, and on these beds, lie some small lakes, from which the springs are supposed to derive their principal supply of water, which, after percolating through the above beds and dissolving large quantities of their constituents, finds its exit near the base of the mountain Alt cae Coch, where it issues from the slate bed (Black band), and between it and the ironstone. From the above data the composition of the water is easily accounted for. There are several pyrites-mines in the vicinity, one of which is situated just over the springs, but much further up the mountain-side.

The author has been indebted, for Dr. Hassall's analysis and some of his remarks relative to the geological position of the springs, to the pamphlet of Dr. Hayward previously mentioned.

XLI. *On the Colour of Nankin Cotton.*

By Edward Schunck, Ph.D., F.R.S.

Read November 18th, 1873.

Among the numerous varieties of cotton existing in commerce there is one which cannot fail to strike the most unpractised eye, in consequence of the peculiar colour (varying from a pale yellow, or, rather, fawn, to brown or reddish brown) which it exhibits. This kind of cotton is generally called "Nankin" cotton, from its having been used by the Chinese for the manufacture of the well-known fabric called nankin or nankeen. Specimens of raw cotton of the colour referred to from other countries, however, such as India, America, the west coast of Africa, and the shores of the Mediterranean, are found in all extensive collections of the fibre; so that it cannot be considered a product peculiar to China. In Malta, I am informed, it is especially abundant, more so even than the ordinary white kind. Whether it is produced by a peculiar species or variety of the cotton-plant or whether the colour is owing to peculiarities of climate, soil, or method of culture influencing the plant is, on the other hand, a question not easily determined. Considerable doubt, indeed, prevails as to the number of species embraced by the genus *Gossypium*, and the characters by which they are distinguished from one another—some authorities admitting only four species, whilst others describe more than twenty. Among the former is Dr. Forbes Royle, who says*:—"The result of our investigation of the species of the genus *Gossypium* is, that there are at least four distinct species, which may be easily distinguished, and that the great mass, probably the whole,

* On the Culture and Commerce of Cotton &c., p. 151.

of the cotton of commerce is yielded by three of these species and their varieties." As a botanical characteristic, the colour of the fibre yielded by the plant seems to be of little importance. Any one, indeed, examining a collection of specimens of cotton must see that there are few marked distinctions between them as regards colour, none being absolutely white, while most of them exhibit various shades of cream-colour, verging to fawn. Nankin cotton may be considered to be situated, as far as colour is concerned, at one end of the scale, at the other end of which we find Sea-Island and other almost pure white kinds. Several authorities assert, it is true, that Nankin cotton is produced by one species of the genus only, viz. *Gossypium religiosum*; but others say it is found on more than one species. Dr. Forbes Royle, who upholds the latter opinion, says:—"*G. religiosum* of Linnæus seems to be distinguished from other species only by having tawny-coloured cotton; but we have seen that both the common Indian cotton, the Chinese cotton, the arboreous species, and *G. barbadense* all occasionally produce nankeen-coloured cotton, and that therefore it cannot be considered as characteristic." Referring to "China cotton," the same author says* :—"The specimen in 'Herb. Hook.,' from Mr. Fortune, is less hairy than most Indian specimens, though clothed with a number of short hairs. Mr. Fortune states, in a note with some specimens that he sent to Dr. Lindley from China, that the white-coloured and the nankeen-coloured cotton are yielded by the same plant, and that the two kinds are separated by the Chinese. Besides India and China, this species is cultivated in Persia, Syria, Asia Minor, and the islands of the Mediterranean, as well as in the north of Africa and the south of Europe. The kind yielding the nankeen-coloured cotton in Malta is probably a variety." Fortune, in his

* On the Culture and Commerce of Cotton &c., p. 143.

Travels*, makes the following statement regarding the cotton-plant of China:—"The Chinese or Nanking cotton-plant is the *Gossypium herbaceum* of botanists, and the 'Mie wha' of the northern Chinese. It is a branching annual, growing from 1–3 or 4 feet in height, according to the richness of the soil, and flowering from August to October. . . . . The yellow cotton, from which the beautiful Nanking cloth is manufactured, is called 'Tze mie wha' by the Chinese, and differs but slightly in its structure and general appearance from the kind just noticed. I have often compared them in the cotton-fields where they were growing; and although the yellow variety has a more stunted habit than the other, it has no characters which constitute a distinct species. It is merely an accidental variety; and although its seeds may generally produce the same kind, they doubtless frequently yield the white variety, and *vice versâ*. Hence specimens of the yellow cotton are frequently found growing amongst the white in the immediate vicinity of Shanghai; and, again, a few miles northward, in the fields near the city of Poushan, on the banks of the Yang-tze-Kiang, where the yellow cotton abounds, I have often gathered specimens of the white variety." The opinion here expressed is also that of Parlatore†, who affirms without hesitation that the plant bearing red or reddish cotton is merely a variety either of *G. arboreum* or *G. hirsutum*.

Mr. Thomas Clegg, of Manchester, whose knowledge of the properties, commercial value, and places of growth of the various kinds of cotton is probably unrivalled, has kindly communicated to me his views on this subject. Mr. Clegg says:—"I found Nankin cotton abundantly at Malta, many parts of Tunis, and in great quantities on the west coast of Africa. Dr. Livingstone has sent me

* Two Visits to the Tea-countries of China, vol. i. p. 199.

† Le Specie dei Cotoni. Firenze: 1866.

many samples of it; and I have frequently had specimens of it from other, but always arid, dry, and hot parts of the world. The Maltese, however, has always been the best. It is very short in staple, coarse, and of little value in itself, especially as so little of it is produced. Being high-coloured, of course, if used alone, it would give a high peculiar colour to the cloth; and as mixing it with whiter cotton generally stripes and spoils the cloth, it is in very bad repute. In China and Japan it gets more dusky and dark, and even lower in staple. On the west coast of Africa it seems to be hybridized and modified in colour; the seed, when cleaned from the cotton, is generally only half clothed with the fibre, the other half being black. But whether Nankin-coloured or a little whiter, it is always on that coast much longer in staple, and, though rather coarse, still a good useful cotton. Nankin cotton is always, so far as I have seen, from fibrous-coated seed. According to my experience, it is only in very hot countries, and on a rather arid soil, that the really dark Nankin is produced. I think, if experiments were tried for three or four years together, Maltese on the west coast of Africa would resemble African, and West-African seed sown at Malta would become Maltese cotton; and I almost think that West-African seed, which at home produces yellow-tinged cotton, would, in one or two years, in the New-Orleans district produce white cotton. I further think that pure New-Orleans seed, sown at Malta, in three or four years would give cotton of the red tinge of ordinary Maltese." Mr. Clegg seems therefore to agree with those who think that the variations in colour observed in cotton are entirely owing to differences of climate and soil, and are not peculiarities attaching to different species of the plant.

I may further quote a few observations on this kind of cotton from a communication on the subject by Lieut.-Col. R. Trevor Clarke, who writes as follows:—"The nankeen

colour in cotton staple may be considered, I think, a normal variational state, rarely met with in the present day, but very probably natural to the wild forms now nearly extinct and very imperfectly known. The two plants which have the best claim to be considered wild species that I have met with, namely the Polynesian plant of Nuttall and the São-Paulo one of Mr. Aubertin have both of them yellow cotton. The coloured state, however, is common to several cottons, probably to all. We find it in the hill-cotton of Assam, and in samples from China (Ningpo)—both *Gossypium herbaceum* proper; and I am told it is not unfrequently found in the fields of this species in India and elsewhere. In India, however, the sort actually in cultivation is the yellow form of *G. hirsutum* (Orleans); and my samples from Malta are of the same kind. The only use that I know of for this staple is to make the cotton-blanket clothing of the Afghans; and I think also the Kabyles use it for the same purpose. It seems to be an object of regard and veneration amongst the aborigines of various countries. In Peru the country people weave a striped cloth, white and yellow, from it; and the bodies of their ancient princes (the Incas) were found to have been buried enveloped in the rich brown wool of a coloured form of the large native plant. The paler brown staple coming from Africa is the produce of a coarse kind of Egyptian, and is of stronger quality than most. The curious kidney-cotton also assumes this appearance, as I have samples of it from Parahyba del Norte, and have raised plants from its seeds. In all cases the staple suffers an unfavourable change when assuming this state, and is invariably more or less weak and short."

The experiments, of which I now propose to give a short account, were made in order to ascertain the cause of the peculiar colour by which Nankin cotton is characterized.

The colour of Nankin cloth having been successfully imitated by depositing oxide of iron on and within the cotton fibres in the manner well known to dyers, it might be supposed that the colour of the cotton was due to iron in some form. A few simple experiments, however, suffice to prove that this is not the case. The cotton, on being incinerated, leaves an ash which does not contain a larger proportion of oxide of iron than that of ordinary cotton. Moreover the colour is not removed by treating the cotton with dilute mineral acids capable of dissolving oxide of iron, while it disappears, though slowly, on treatment with boiling alkaline lye.

Another method of imitating the colour consists in mordanting the fabric with alum and then dyeing with oak bark, the process resembling that by which calico is ordinarily dyed of a yellow or fawn colour. It is evident, however, from its resisting the action of acids, that the colour of Nankin cotton cannot be due to the presence of a lake of alumina or any other base.

In order to arrive at some conclusion regarding the nature of the colouring-matter, it was necessary to employ large quantities of material; for though the colour looks intense when the cotton is viewed in mass, it is in reality produced by a small quantity of substance spread over a large extent of surface. I therefore had recourse to the plan adopted on a previous occasion, and described in a paper read before the Society several years ago*. A quantity of yarn made entirely of Nankin cotton from the coast of Coromandel (generally called in Lancashire "Coconado cotton," from the port of shipment) was treated with boiling alkaline lye for several hours; and the resulting dark brown liquid was drawn off and mixed with an excess of acid, which produced a dark brown flocculent precipitate. This was filtered off, washed with water, and then

* Memoirs, 3rd Series, vol. iv. p. 95.

treated in the manner described in the paper just referred to. It was found to consist of the same substances as the precipitate obtained from the alkaline lye with which ordinary Indian or American cotton has been treated, viz.:— *cotton wax*, fusing at the same temperature and having the same properties as that from ordinary cotton; a crystalline *fatty acid*, apparently identical with that previously obtained; *pectic acid*, *parapectic acid*, and *colouring-matters*. It is to the latter that the cotton owes its colour; for this colour is removed to a great extent by treatment with alkali, and is again found tinging the precipitate produced on the addition of acid; and I therefore examined them with more care than the other constituents of the precipitate. Of these colouring-matters two may be distinguished. One of them is easily soluble in alcohol and is obtained on evaporating the solution as a dark brown, shining, transparent resin. The other is almost insoluble in cold alcohol, but dissolves in boiling alcohol, and is deposited, on the solution cooling, in the form of a light-brown powder. Their general properties are the same as those of the analogous colouring-matters from ordinary cotton. They consist, like these, of carbon, hydrogen, nitrogen, and oxygen; but the relative amounts of the constituents are slightly different. The analysis of the colouring-matter easily soluble in alcohol led to the following results:—

0·4378 grm. of the substance, dried at 100° C., gave 0·9346 grm. carbonic acid and 0·2138 grm. water.

0·9958 grm., burnt with soda-lime, gave 0·5920 grm. double chloride of platinum and ammonium.

In 100 parts it contained, therefore,

C	58·22
H	5·42
N	3·73
O	32·63

The other colouring-matter, on being analyzed, gave the following results:—

0·4450 grm., dried at 100° C., gave 0·9374 grm. carbonic acid, and 0·2237 grm. water.

0·7936 grm. gave 0·6288 grm. double chloride of platinum and ammonium.

1·1854 grm. left, on being incinerated, 0·0054 grm. of ash* = 0·45 per cent.

After correction for the ash, these numbers correspond in 100 parts, to

C	57·70
H	5·60
N	4·99
O	31·71

As regards their composition, these colouring-matters do not, I think, differ more widely from the substances prepared by the same process from ordinary cotton than is frequently the case with bodies of the purity of which, in consequence of their not assuming a crystalline form, one can never be perfectly sure. The conclusion at which I arrive, therefore, is that the colour of Nankin cotton is due to the presence of bodies which are, if not identical with, at least very similar to those which cause the much fainter colour of the ordinary kinds. These experiments show, too, that the substances accompanying the cellulose (whether clothing the fibres or contained in their interior) are the same in this variety as in those previously examined.

* The ash was neutral to test-paper and of a reddish colour; it consisted of alumina with traces of lime and oxide of iron.

Fig. 4

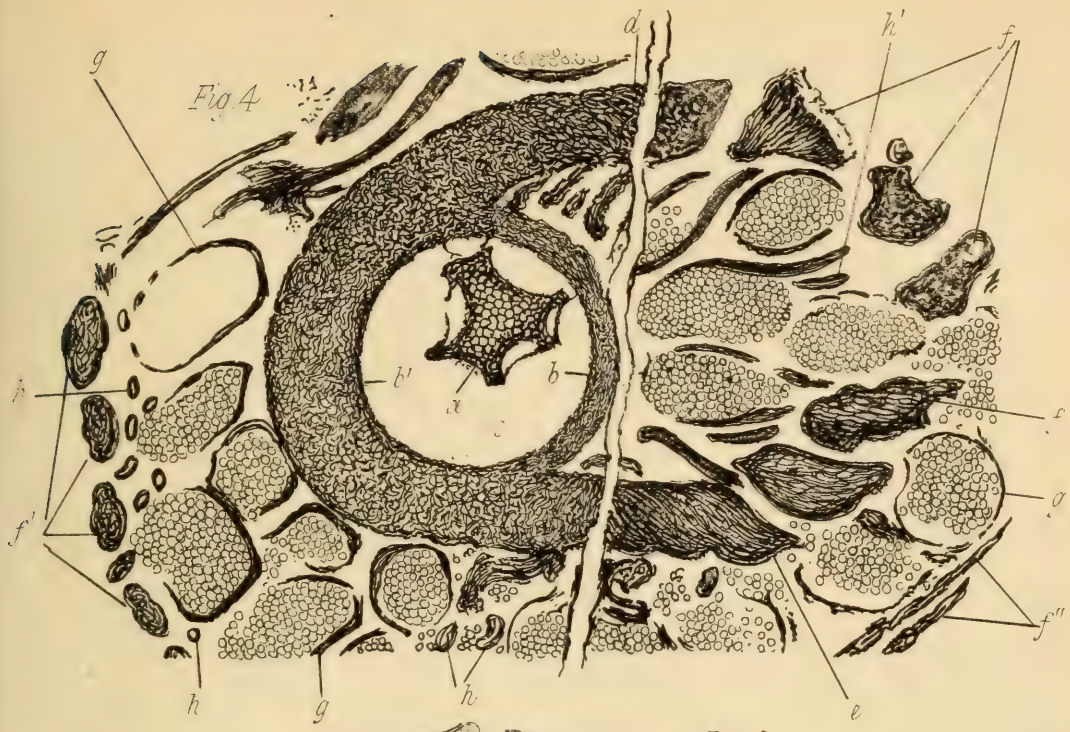
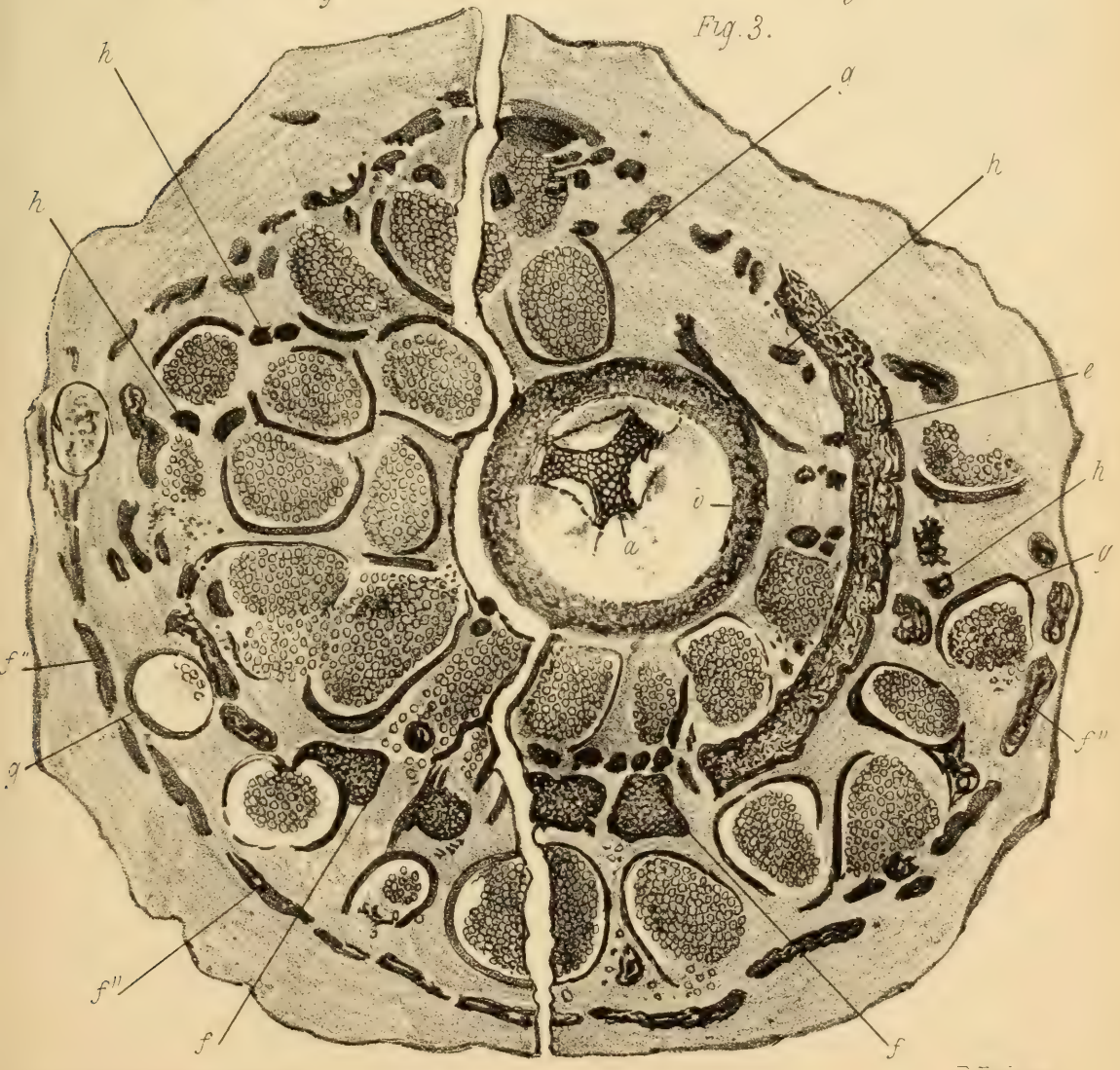
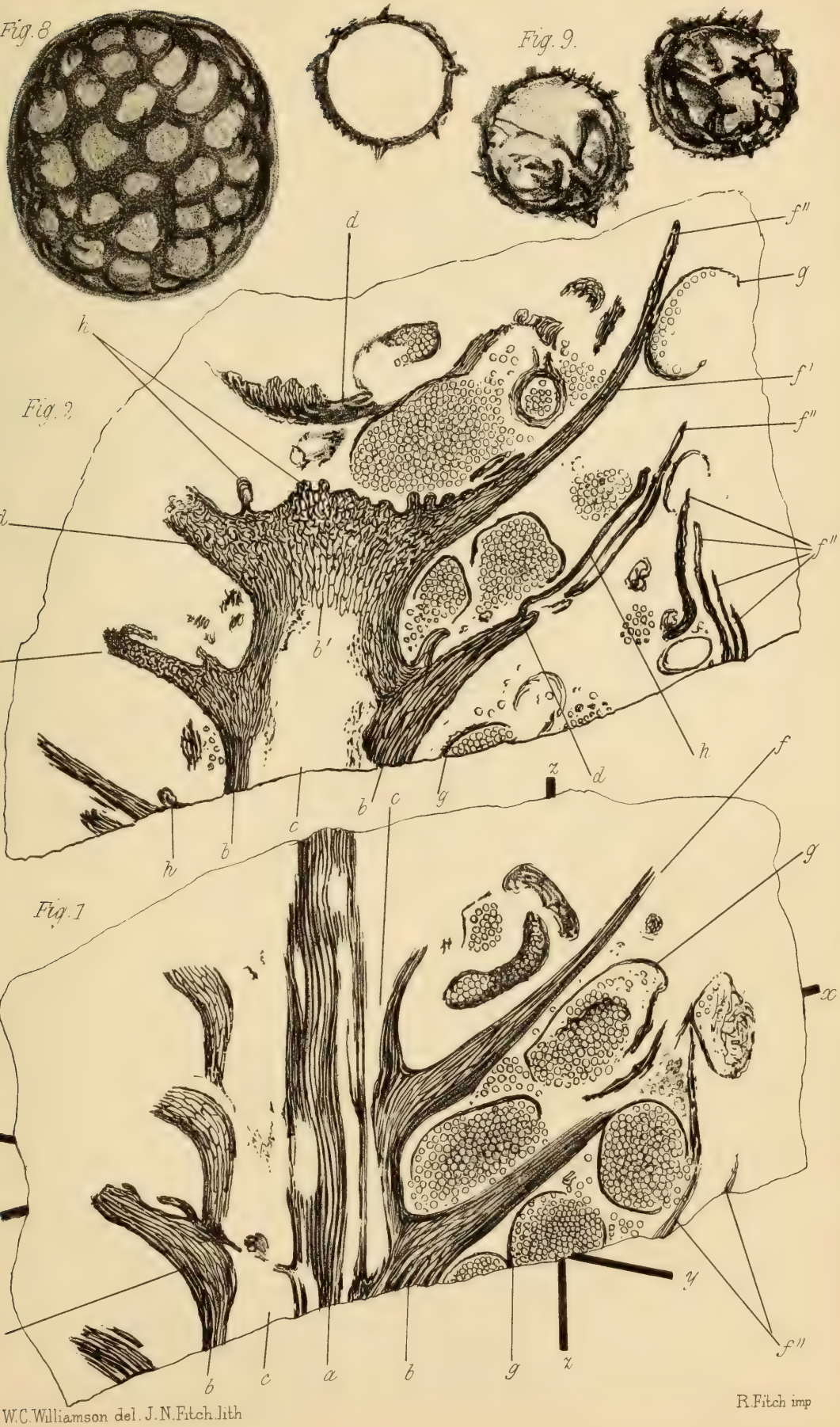


Fig. 3.



W. West, Williamson del. J.N. Fitch, lith.

R. Fisher, imp.



W.C. Williamson del. J.N. Fitch lith.

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Fig. 7.

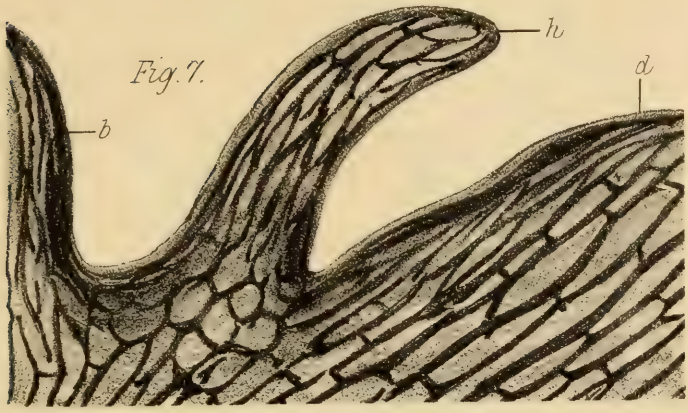

Fig. 5.

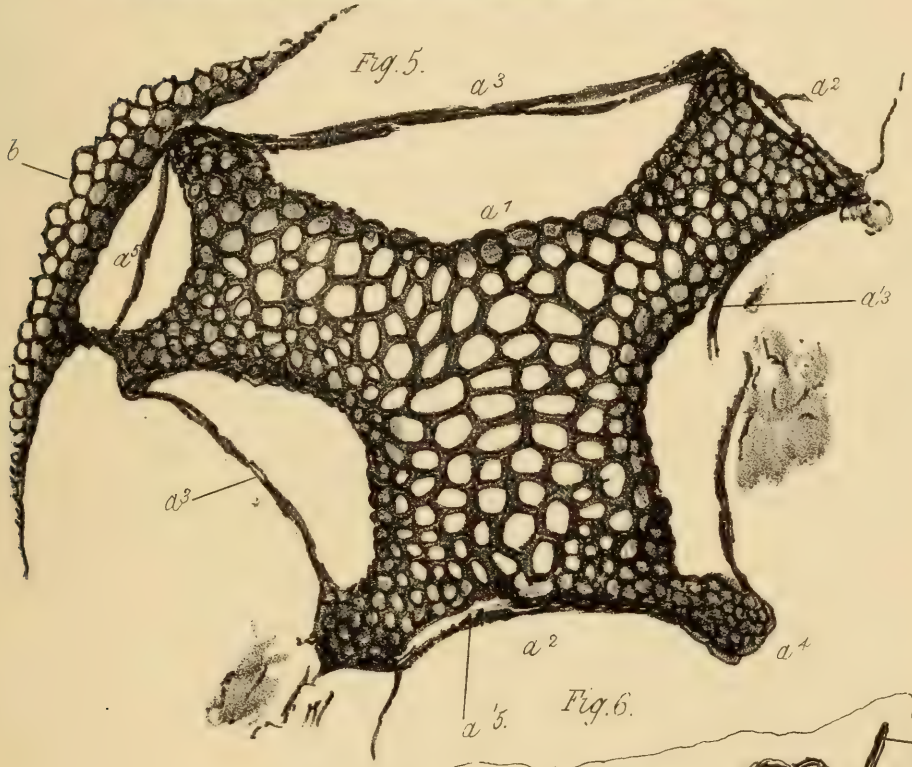

Fig. 6.

W.C.Williamson del. J.N.Fitch lith.

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1843, Apr. 18. Airy, Sir George Biddell, M.A., D.C.L., F.R.S., Astronomer Royal, V.P.R.A.S., Hon. Mem. R.S.E., R.I.A., M.C.P.S., Chev. of the Prussian Order "*Pour le Mérite*," Corr. Mem. Nat. Inst. Washington, U.S., Imper. Inst. France, Imper. Acad. Sc. Petersburg and Roy. Acad. Sc. Berlin, Mem. Acad. Sc. and Lit. Palermo, Roy. Acadd. Sc. Stockholm and Munich, Roy. Soc. Sc. Copenhagen, and Amer. Acad. Arts and Sc. Boston. *The Royal Observatory, Greenwich, London,* S.E.

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1860, Apr. 17. Bunsen, Robert Wilhelm, Ph.D., For. Mem. R.S., Prof. of Chemistry at the Univ. of Heidelberg. *Heidelberg.*

1859, Jan. 25. Cayley, Arthur, M.A., F.R.S., F.R.A.S. *Garden House, Cambridge.*

1866, Oct. 30. Clifton, Robert Bellamy, M.A., F.R.A.S., Professor of Natural Philosophy, Oxford. *New Museum, Oxford.*

1868, Apr. 28. Darwin, Charles, M.A., F.R.S., F.G.S., F.L.S. *Bromley, Kent.*

1844, Apr. 30. Dumas, Jean Baptiste, Gr. Off. Legion of Honour, For. Mem. R.S., Mem. Imper. Instit. France, &c. 42 *Rue Grenelle, St. Germain, Paris.*

DATE OF ELECTION.

1869, Mar. 9. Frankland, Edward, Ph.D., F.R.S., Prof. of Chemistry in the Royal School of Mines, Mem. Inst. Imp. (Acad. Sci.) Par., Corr. Soc. Nat. Scrutat. Helvet., Acad. Reg. Monac. Socius. 14 *Lancaster Gate, Hyde Park, London*, W.

1860, Jan. 24. Fries, Elias, A.M., Prof. Emer. at the Univ. of Upsala, Comm. of the Swedish Order of the Northern Star, Chev. of the Danish Order of "*Dannebrog*," Mem. Swed. Acad., Roy. Acad. Sc. Stockholm and Roy. Soc. Sc. Upsala, &c. *Upsala.*

1843, Feb. 7. Frisiani, nobile Paolo, Prof., late Astron. at the Observ. of Brera, Milan, Mem. Imper. Roy. Instit. of Lombardy, Milan, and Ital. Soc. Sc. *Milan.*

1853, Apr. 19. Hartnup, John, F.R.A.S. *Observatory, Liverpool.*

1867, Apr. 30. Henry, Joseph, Professor, Secretary of the Smithsonian Institution, Washington, U.S.

1848, Jan. 25. Hind, John Russell, F.R.S., F.R.A.S., Superintendent of the Nautical Almanack.

1866, Jan. 23. Hofmann, A. W., LL.D., Ph.D., F.R.S., F.C.S., Ord. Leg. Hon. S^{rum} Lazar. et Maurit. Ital. Eq., Inst. Imp. (Acad. Sci.) Par., Acad. Imp. Sci. Petrop., Imp. Reg. Vindob., Acad. Reg. Sci. Amstelod., Berol., Monach., Taur., Soc. Philomat. Paris., Phil. Amer. Philad., Phys. Francof., Bat. Roterod., Nat. Neo-Granad., Nat. Halæ, Univ. Cas., Phys. Med. Erlang., Indust. Mulh. Corresp. 10 *Dorotheenstrasse, Berlin.*

1869, Jan. 12. Huggins, William, F.R.S., Pres. R.A.S. *Upper Tulse Hill, Brixton, London*, S.W.

1872, Apr. 30. Huxley, Thomas Henry, LL.D. (Edin.), Ph.D., F.R.S., Professor of Natural History in the Royal School of Mines, South Kensington Museum, F.G.S., F.Z.S. F.L.S., Stell. Pol. Suec. Eq., Acad. Imp. Sci. Petrop., Reg. Sci. Berol., Monach. et Holm., Cæs. Nat. Cur. Dresd., Nat. Sci. Philad. et Buff. Soc., Reg. Sci. Gott. et Soc. Anthrop. Par. Corr., Acad. Reg. Hib. et Inst. Egypt. Soc. Honor. *School of Mines, South Kensington Museum*, S.W., and 4 *Marlborough Place, Abbey Road*, N.W.

1852, Oct. 16. Kirkman, Rev. Thomas Penyngton, M.A., F.R.S. *Croft Rectory, near Warrington.*

1848, Oct. 31. Lassell, William, F.R.S., F.R.A.S., Hon. Mem.

DATE OF ELECTION.

R.S.E., Hon. Mem. Philomath. Soc. Paris. *Ray Lodge, Maidenhead.*

1847, Apr. 20. Le Verrier, Urbain Jean Joseph, For. Mem. R.S., Comm. Legion of Honour, Mem. Imper. Instit. France, &c. *L'observatoire Impérial, Paris.*

1872, Apr. 30. Lloyd, Rev. Humphrey, D.D., M.R.I.A., F.R.S., Provost of Trinity College, Dublin, Corr. Mem. Roy. Soc. Sc. Göttingen. *Trinity College, Dublin.*

1854, Jan. 24. Morin, Arthur, Gr. Off. Legion of Honour, General of Brigade, Mem. Imper. Instit. France, formerly élève Polytechn. School, Dir. Conserv. of Arts, Paris, Corr. Mem. Roy. Acadd. Sc. Berlin, Madrid, and Turin, Acad. Georg. Florence, Imper. Acad. Metz, and Industr. Soc. Mulhouse. 3 *Rue des Beaux-arts, Paris.*

1844, Apr. 30. Owen, Richard, M.D., LL.D., F.R.S., F.L.S., F.G.S., V.P.Z.S., Director of the Nat. Hist. Department, British Museum, Hon. F.R.C.S. Ireland, Hon. M.R.S.E., For. Assoc. Imper. Instit. France, Mem. Imper. Acadd. Sc. Vienna and Petersburg, Imper. Soc. of Naturalists Moscow, Roy. Acadd. Sc. Berlin, Turin, Madrid, Stockholm, Munich, Amsterdam, Naples, Brussels and Bologna, Roy. Socc. Sc. Copenhagen and Upsala, and Amer. Acad. Arts and Sc. Boston, Corr. Mem. Philom. Soc. Paris, Mem. Acad. Georg. Florence, Soc. Sc. Haarlem and Utrecht, Soc. of Phys. and Nat. Hist. Geneva, Acad. dei Nuovi Lincei, Rome, Roy. Acadd. Sc. Padua, Palermo, Acad. Gioen. Catania, Phys. Soc. Berlin, Chev. of the Prussian Order "*Pour le Mérite*," For. Assoc. Instit. Wetter., Philadelphia, New York, Boston, Impér. Acad. Med. Paris, and Imper. and Roy. Med. Soc. Vienna. *British Museum, London,* W.C.

1851, Apr. 29. Playfair, Rt. Hon. Lyon, C.B., Ph.D., F.R.S., F.G.S., F.C.S., Professor of Chemistry Univ. Ed. 4 *Queensbury-place, South Kensington, London,* S.W.

1866, Jan. 23. Prestwich, Joseph, F.R.S., F.G.S. *Shoreham, near Sevenoaks.*

1866, Jan. 23. Ramsay, Andrew Crombie, F.R.S., F.G.S., Director of the Geological Survey of Great Britain, Professor of Geology, Royal School of Mines, Ord.

DATE OF ELECTION.

S^rum Maur. et Lazar. Eq., Amer. Phil. Soc. Philad. Socius, et Nat. Sc. Soc. Ital. Socius Corresp., &c. *Geological Survey Office, Jermyn-street, London,* S.W.

1849, Jan. 23. Rawson, Robert. *Havant, Hants.*

1859, Apr. 19. Reichenbach, Carl, Baron von. *Gut Reissenberg, nächst Grinzing, Vienna.*

1844, Apr. 30. Sabine, General Sir Edward, R.A., D.C.L., Treas. and P.R.S., F.R.A.S., Hon. Mem. C.P.S., Chev. of the Prussian Order "*Pour le Mérite,*" Mem. Imper. Acad. Sc. Petersburg, Roy. Acadd. Sc. Berlin, Brussels and Göttingen, Roy. Soc. Sc. Drontheim, Acad. Sc. Philadelphia, Econ. Soc. Silesia, Nat. Hist. Soc. Lausanne and Roy. Batavian Soc., Corr. Mem. Roy. Acad. Sc. Turin, Nat. Instit. Washington, U.S., Geogr. Socc. Paris, Berlin, and Petersburg. 13 *Ashley-place, Westminster, London,* S.W.

1872, Apr. 30. Sachs, Julius, Ph.D. *Prague.*

1872, Apr. 30. Schimper, W. P., Ph.D., Conservator of the Museum of Nat. Hist. Strasburg. *Strasburg.*

1869, Dec. 14. Sorby, Henry Clifton, F.R.S., F.G.S., Soc. Min. Petrop. Socius, Acad. Sci. Nat. Philad. et Lyc. Hist. Nat. Nov. Ebor. Corr. Mem. *Broomfield, Sheffield.*

1872, Apr. 30. Stenhouse, John, LL.D., F.R.S., V.P.C.S. 17 *Rodney-street, Pentonville,* N.

1851, Apr. 29. Stokes, George Gabriel, M.A., D.C.L., Sec. R.S., Lucasian Professor of Mathem. Univ. Cambridge, F.C.P.S., Mem. Batav. Soc. Rotterdam, Corr. Mem. Roy. Acad. Sc. Berlin. *Pembroke College, Cambridge.*

1861, Jan. 22. Sylvester, James Joseph, M.A., F.R.S., Professor of Mathematics. 60 *Maddox-street. London,* W.

1868, Apr. 28. Tait, Peter Guthrie, M.A., F.R.S.E., Professor of Natural Philosophy, Edinburgh.

1851, Apr. 22. Thomson, Sir William, M.A., D.C.L., LL.D., F.R.SS. L. and E., Prof. of Nat. Philos. Univ. Glasgow. 2 *College, Glasgow.*

1872, Apr. 30. Trécul, A., Member of the Institute of France. *Paris.*

1868, Apr. 28. Tyndall, John, LL.D., F.R.S., F.C.S., Professor of Natural Philosophy in the Royal Institution and Royal School of Mines, Acadd. et Socc. Philomath. Par., Reg. Sci. Götting., Holland., Harl., Phys. et Nat. Hist. Genev., Nat. Ges. Tigur., Halæ, Marburg,

DATE OF ELECTION.

Vratisl., Upsal., Nat. Sc. Berol. Socius. *Royal Institution, London,* W.

1872, Apr. 30. Watson, Hewett Cottrell, F.L.S. *Thames Ditton, Surrey.*

1850, Apr. 30. Woodcroft, Bennet, F.R.S., Professor, Superint. of Regist. of Patents. *Southampton Buildings, London,* W.C.

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1861, Jan. 22. Buckland, George, Professor, University College, Toronto. *Toronto.*

1867, Feb. 5. Cialdi, Alessandro, Commander, &c. *Rome.*

1870, Mar. 8. Cockle, The Hon. Sir James, M.A., F.R.S., F.R.A.S., F.C.P.S., Chief-Justice of Queensland. *Brisbane, Queensland, Australia.*

1866, Jan. 23. De Caligny, Anatole, Marquis, Corresp. Mem. Acadd. Sc. Turin and Caen, Socc. Agr. Lyons, Sci. Cherbourg, Liége, &c.

1861, Apr. 2. Durand-Fardel, Max, M.D., Chev. of the Legion of Honour, &c. 36 *Rue de Lille, Paris.*

1849, Apr. 17. Girardin, J., Off. Legion of Honour, Corr. Mem. Imper. Instit. France, &c. *Lille.*

1862, Jan. 7. Gistel, Johannes Franz Xavier, Ph.D., late Prof. of Nat. Hist. and Geogr., Libr. Secr. and Conserv. at the Museum of Nat. Hist. Regensburg, Corr. Mem. Imper. Roy. Geol. Inst. Vienna, Acadd. and Socc. Sc. Cherbourg, Caen, Dijon, Aix, Orleans, Angers, Brussels, Rheims, Nantes, Antwerp, Linnean Socc. Caen, Angers, Marseilles, La Rochelle and Paris. 19 *Steinweg, Regensburg, Bavaria.*

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1816, Apr. 26. Kenrick, Rev. John, M.A. *York.*

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1862, Jan. 7. Lancia di Brolo, Federico, Duc. Inspector of Studies, &c. *Palermo.*

1859, Jan. 25. Le Jolis, Auguste-François, Ph.D., Archiviste perpétuel and late President of the Imper. Soc. Nat. Sc. Cherbourg, Mem. Imp. Leop.-Car. Acad. Nat. Sc., Imp. Soc. Naturalists Moscow, Acad. Nat. Sc. Philadelphia, Roy. Botan. Socc. Regensburg, Leiden, Edinburgh, Botan. Soc. Canada, Linnean Socc. Lyon, Bordeaux, and Caen, Physiogr. Soc. Lund, Imp. Roy. Geol. Instit. Vienna, Imp. Roy. Zool. and Botan. Soc. Vienna, Roy. Acad. Sc. Lucca and Prague, Imp. Acadd. Sc. and Lit. Chambéry, Toulouse, Rouen, Caen, Lille, &c., Acad. Socc. Cherbourg and Angers, Hortic. Soc. Cherbourg, Roy. Acad. Archæol. Brussels, Socc. Nat. Sc. Catania, Athens, Boston, Dorpat, Riga, &c. *Cherbourg.*

1857, Jan. 27. Lowe, Edward Joseph, F.R.S., F.R.A.S., F.G.S., Mem. Brit. Met. Soc., Hon. Mem. Dublin Nat.Hist. Soc., Mem. Geol. Soc. Edinburgh, &c. *Nottingham.*

1862, Jan. 7. Nasmyth, James, C.E., F.R.A.S., &c. *Penshurst, Tunbridge.*

1851, Apr. 29. Pincoffs, Peter, M.D., Knt. of the Turkish Order of the "*Medjidié*" 4th Cl., Mem. Coll. Phys. London, Brussels, and Dresden, Hon. and Corr. Mem. Med. and Phil. Socc. Antwerp, Athens, Brussels, Constantinople, Dresden, Rotterdam, Vienna, &c. *Naples.*

1869, Jan. 12. Saint Venant, Barré de, Ingénieur en chef des Ponts et Chaussées, Corr. Soc. Sci. de Lille et de l'Acad. Romaine des *Nuovi Lincei*, Mem. Soc. Philomatique.

1867, Feb. 5. Schönfeld, Edward, Ph.D., Director of the Mannheim Observatory.

1834, Jan. 24. Watson, Henry Hough. *Bolton, Lancashire.*

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1861, Jan. 22. Alcock, Thomas, M.D., Extr. L.R.C.P. Lond., M.R.C.S. Engl., L.S.A. *Oakfield, Ashton-on-Mersey.*

1873, Jan. 7. Allmann, Julius. 27 *Corporation-street.*

1870, Dec. 13. Angell, John. *Manchester Grammar School.*

1861, Jan. 22. Anson, Ven. Archd. George Henry Greville, M.A. *Birch Rectory, Rusholme.*

1837, Aug. 11. Ashton, Thomas. 36 *Charlotte-street.*

1871, Oct. 31. Ashworth, John. *Earleswood, Seedley, Pendleton.*

1874, Nov. 3. Axon, William E. A., M.R.S.L., F.S.I. 4 *Victoria Terrace, Rusholme.*

1865, Nov. 15. Bailey, Charles. 85 *Withington-road, Whalley Range.*

1824, Jan. 23. Barbour, Robert. 18 *Aytoun-street.*

1867, Nov. 12. Barrow, John. 3 *Egerton-terrace, Birch-lane, Longsight.*

1858, Jan. 26. Baxendell, Joseph, F.R.A.S., Corr. Mem. Roy. Phys. Econ. Soc. Konigsberg, and Acad. Sc. & Lit. Palermo. 32 *Crescent-road, Cheetham Hill.*

1847, Jan. 26. Bazley, Sir Thomas, Bart., M.P. *Eynsham Hall, Oxford.*

1847, Jan. 26. Bell, William. 51 *King-street.*

1870, Nov. 15. Bennion, John A., F.R.A.S. 88 *Pollard-street.*

1858, Jan. 26. Benson, Davis. 4 *Chester-street.*

1868, Dec. 15. Bickham, Spencer H., jun. *Endsleigh, Alderley Edge.*

1842, Jan. 55. Binney, Edward William, F.R.S., F.G.S., Hon. Mem. Geol. Socs. Edinburgh and Liverpool, and Geol. and Polytech. Soc. West Riding of Yorkshire. 55 *Peter-street.*

1870, Nov. 15. Bird, John Durham, M.D. *Heaton Chapel.*

1821, Jan. 26. Blackwall, John, F.L.S. *Hendre, Llanrwst.*

1861, Jan. 22. Bottomley, James, D.Sc., B.A., F.C.S. 7 *Irwell-terrace, Lower Broughton.*

1839, Oct. 29. Bowman, Henry. *Brooklands, Brockham Green, Reigate, London.*

1875, Nov. 16. Boyd, John. *Princes-road, Fallowfield.*

1873, Nov. 4. Bridson, Joseph Ridgway, F.R.A.S. *Bolton.*

1855, April 17. Brockbank, William, F.G.S. *St. Andrew's Chambers, Albert-square.*

1861, April 2. Brogden, Henry. *Hale Lodge, Altrincham.*

1844, Jan. 23. Brooks, William Cunliffe, M.A., M.P. *Bank,* 92 *King-street.*

DATE OF ELECTION.

1860, Jan. 24. Brothers, Alfred, F.R.A.S. 14 *St.-Ann's-square.*

1867, Dec. 10. Broughton, Samuel. *Heaton-on-Mersey.*

1846, Jan. 27. Browne, Henry, M.D., M.A., M.R.C.S. Engl. 244 *Oxford-street.*

1872, Nov. 12. Burghardt, Charles Anthony, Ph.D. 110 *King-street.*

1874, Dec. 15. Carrick, Joseph. *St. Andrew's Chambers, Albert-square.*

1858, Jan. 26. Casartelli, Joseph. 43 *Market-street.*

1852, April 20. Chadwick, David, F.S.S., Asso. Inst. C.E., M.P. 64 *Cross-street Chambers.*

1842, Jan. 25. Charlewood, Henry. 5 *Clarence-street.*

1854, April 18. Christie, Richard Copley, M.A., Prof. Hist. Owens College. 2 *St.-James's-square.*

1841, April 30. Clay, Charles, M.D., Extr. L.R.C.P. London, L.R.C.S. Edin. 101 *Piccadilly.*

1853, Jan. 25. Cottam, Samuel, F.R.A.S. 6 *Essex-street.*

1859, Jan. 25. Coward, Edward. *Heaton Mersey, near Manchester.*

1861, Nov. 12. Coward, Thomas. *Bowdon.*

1831, April 29. Crompton, Samuel, M.R.C.S. Eng., L.S.A., F.R. Med. Chir. Soc. 24 *St.-Ann's-square.*

1848, Jan. 25. Crowther, Joseph Stretch. 20 *St.-John's-street.*

1861, April 2. Cunningham, William Alexander. *Stanley Grove, Oxford-street.*

1854, Feb. 7. Dale, John, F.C.S. *Cornbrook Chemical Works, Chester-road.*

1871, Nov. 8. Dale, Richard Samuel, B.A. *Cornbrook Chemical Works, Chester-road.*

1842, April 19. Dancer, John Benjamin, F.R.A.S. 43 *Cross-street.*

1863, Feb. 10. Darbishire, George Stanley. 14 *John-Dalton-street.*

1853, April 19. Darbishire, Robert Dukinfield, B.A., F.G.S. 26 *George-street.*

1869, Nov. 2. Dawkins, William Boyd, M.A., F.R.S. *Museum, Owen's College.*

1870, Nov. 15. Deacon, Henry, F.C.S. *Alkali Works, Widnes.*

1861, Dec. 10. Deane, William King. 25 *George-street.*

1855, Jan. 23. Dickinson, William Leeson. 1. *St.-James's-street.*

1872, April 16. Freeston, Rev. Joseph. *Chapel-lane, Blackley.*

1873, Nov. 18. Gamgee, Arthur, M.D., F.R.S., Professor of Practical Physiology and Histology. *Owens College.*

1840, Jan. 21. Gaskell, Rev. William, M.A. 46 *Plymouth-grove.*

DATE OF ELECTION.

1874, Nov. 3. Grimshaw, Harry, F.C.S. 110 *King-street.*

1875, Feb. 9. Gwyther, R. F., B.A., Lecturer on Mathematics, Owens College. *Owens College.*

1871, Oct. 3. Harrison, Thomas. 18 *St.-Ann's-square.*

1862, Nov. 4. Hart, Peter. 150 *London-road.*

1839, Jan. 22. Hawkshaw, Sir John, F.R.S., F.G.S., Mem. Inst. C.E. 33 *Great George-street, Westminster, London, S.W.*

1873, Dec. 16. Heelis, James. 75 *Princess-street.*

1828, Oct. 31. Henry, William Charles, M.D., F.R.S. 11 *East-street, Lower Mosley-street.*

1868, Nov. 17. Herford, Rev. Brooke. 13 *Upper Camp-street, Higher Broughton.*

1861, April 30. Heys, William Henry. *Wycliffe Villa, Cheadle-road, Stockport.*

1833, April 26. Heywood, James, F.R.S., F.G.S., F.S.A. 26 *Kensington-Palace Gardens, London, W.*

1864, Mar. 22. Heywood, Oliver. *Bank, St.-Ann's-street.*

1851, April 29. Higgin, James. *Little Peter-street, Gaythorn.*

1845, April 29. Higgins, James. *King-street, Salford.*

1839, Jan. 22. Hobson, John. *Rockville, Ballyshannon, Donegal.*

1861, April 2. Hobson, John Thomas, Ph.D. *Monton Lodge, Eccles.*

1854, Jan. 24. Holcroft, George, *St.-Mary's-gate.*

1855, Jan. 23. Holden, Isaac. 64 *Cross-street.*

1846, Jan. 27. Holden, James Platt. *St.-James's Chambers,* 3 *South King-street.*

1873, Dec. 2. Howorth, Henry H., F.S.A. *Derby House, Eccles.*

1857, Jan. 27. Hunt, Edward, B.A., F.C.S. 42 *Quay-street, Salford.*

1855, Jan. 25. Hurst, Henry Alexander. 10 *Peel-street.*

1866, Nov. 13. Jevons, William Stanley, M.A., Professor of Logic, &c. Owens College. *Owens College.*

1872, Feb. 6. Jewsbury, Sidney. *Ellesmere Chambers, King-street.*

1850, April 30. Johnson, Richard, F.C.S. *Oak Bank, Fallowfield.*

1870, Nov. 1. Johnson, William H., B.Sc. 27 *Dale-street.*

1848, April 18. Joule, Benjamin St. John Baptist. *Cliff Point, Higher Broughton.*

1842, Jan. 25. Joule, James Prescott, D.C.L., LL.D., F.R.S., F.C.S., Hon. Mem. C.P.S. and Inst. Eng. Scot., Corr. Mem. Inst. Fr. (Acad. Sc.) Paris, and Roy. Acad. Sc. Turin. *Cliff Point, Higher Broughton.*

1852, Jan. 27. Kennedy, John Lawson. 47 *Mosley-street.*

1867, Nov. 26. Kipping, James Stanley. *Branch Bank of England.*

DATE OF ELECTION.

1862. April 29. Knowles, Andrew. *High Bank, Pendlebury.*

1860, Jan. 24. Latham, Arthur George. *Lower King-street.*
1863, Dec. 15. Leake, Robert. 3 *Bond-street.*
1850, April 30. Leese, Joseph. *Altrincham.*
1871, Oct. 3. Livesey, Thomas. 12 *Tib-lane.*
1857, Jan. 27. Longridge, Robert Bentink. 67 *King-street.*
1870, April 19. Lowe, Charles. 61 *Piccadilly.*
1850, April 30. Lund, Edward, F.R.C.S. Eng., L.S.A. 22 *St.-John's-street.*
1859, Jan. 25. Lynde, James Gascoigne, M. Inst. C.E., F.G.S. *Town Hall.*

1829, Oct. 30. McConnel, James. *Esher, Surrey.*
1838, April 17. McConnel, William. 90 *Henry-street, Oldham-road.*
1866, Nov. 13. McDougall, Arthur. 68 *Port-street.*
1875, Oct. 5. Mackereth, Thomas, F.R.A.S., F.M.S. *Eccles.*
1859, Jan. 25. Maclure, John William, F.R.G.S. *Cross-street.*
1875, Jan. 26. Mann, John Dixon, Licentiate of the King and Queen's College of Physicians, Ireland; and M.R.C.S. Eng. 16 *St.-John's-street.*
1873, Nov. 4. Marshall, Rev. William, B.A. 6 *Swinton Avenue, Plymouth-grove.*
1858, April 20. Mather, Colin. *Iron Works, Deal-street, Brown-street, Salford.*
1864, Nov. 1. Mather, William. *Iron Works, Deal-street, Brown-street, Salford.*
1868, Nov. 17. Mawson, John Isaac, C.E. 4 *Essex-street.*
1837, Jan. 27. Mellor, William. 3 *Hyde-road, Ardwick.*
1873, Mar. 18. Melvill, James Cosmo, M.A., F.L.S. 6 *North Corridor, Royal Exchange.*
1864, Mar. 8. Micholls, Horatio. 6 *Kensington-Garden Terrace, Hyde Park, London, W.*
1861, Oct. 29. Morgan, John Edward, M.B., M.A., M.R.C.P., Lond., F.R. Med. and Chir. S. 1 *St.-Peter's-square.*
1870, Nov. 1. Morris, Walter. 68 *Fountain-street.*
1875, Feb. 9. Muir, M. M. Pattison, F.R.S.E. *Owens College.*

1854. Feb. 7. Nevill, Thomas Henry. 19 *George-street.*
1850, Jan. 24. Newall, Henry. *Hare-hill, Littleborough.*
1873, Mar. 4. Nicholson, Francis, F.Z.S. 62 *Fountain-street.*
1875, Nov. 16. Nix, E. W., M.A. *Branch Bank of England, King-street.*

1862, Dec. 30. Ogden, Samuel. 10 *Back Mosley-street.*

DATE OF ELECTION.

1861, Jan. 22. O'Neill, Charles, F.C.S., Corr. Mem. Ind. Soc. Mulhouse. 254 *Oxford-street.*

1844, April 30. Ormerod, Henry Mere. 5 *Clarence-street.*

1861, April 30. Parlane, James. 33 *Dickinson-street.*

1874, Jan. 13. Pennington, Rooke. *Mawdsley-street, Bolton.*

1870, Nov. 29. Piers, Sir Eustace Fitzmaurice, Bart. *St. George's Iron Works, Hulme.*

1854, Jan. 24. Pochin, Henry Davis. 44 *Quay-street, Salford.*

1860, April 17. Pocklington, Rev. Joseph Nelsey, B.A. *Rectory, St. Michael's, Hulme.*

1861, Jan. 22. Radford, William. 41 *John-Dalton-street.*

1854, Feb. 7. Ramsbottom, John. *Harewood Lodge, Mottram.*

1859, April 19. Ransome, Arthur, M.A., M.D. Cantab., M.R.C.S. 1 *St.-Peter's-square.*

1869, Nov. 16. Reynolds, Osborne, M.A., Professor of Engineering, Owens College. *Owens College.*

1860, Jan. 24. Roberts, William, M.D., B.A., M.R.C.P. Lond. 89 *Mosley-street.*

1864, Dec. 27. Robinson, John. *Atlas Works, Great Bridgewater-street.*

1822, Jan. 25. Robinson, Samuel. *Blackbrook Cottage, Wilmslow.*

1864, Jan. 12. Rogerson, John. *Gaythorn.*

1858, Jan. 26. Roscoe, Henry Enfield, B.A., Ph.D., F.R.S., F.C.S., Professor of Chemistry, Owens College. *Owens College.*

1851, April 29. Sandeman, Archibald, M.A. *Tulloch, near Perth.*

1870, Dec. 13. Schorlemmer, Carl, F.R.S., F.C.S. *Owens College.*

1842, Jan. 25. Schunck, Edward, Ph.D., F.R.S., F.C.S. *Oaklands, Kersal.*

1873, Nov. 18. Schuster, Arthur, Ph.D. *Owens College.*

1852, April 20. Sidebotham, Joseph, F.R.A.S. 19 *George-street.*

1869, Feb. 23. Smart, Robert Bath, M.R.C.S. 176 *Oxford-street.*

1838, Jan. 26. Smith, George Samuel Fereday, M.A., F.G.S. *Grove House, Tunbridge Wells.*

1845, April 29. Smith, Robert Angus, Ph.D., F.R.S., F.C.S., Corr. Mem. I.R. Geol. Inst. Vienna. 22 *Devonshire-street, All Saints.*

1871, Nov. 14. Smith, Watson, jun., F.C.S. *Hawthorn House, Wilmslow.*

1859, Jan. 25. Sowler, Thomas. *Red Lion-street, St.-Ann's-square.*

1851, April 29. Spence, Peter, F.C.S., M.S.A. *Alum Works, Newton Heath.*

DATE OF ELECTION.

1864, Dec. 27. Spencer, Joseph. 105 *Portland-street.*

1870, Nov. 1. Stewart, Balfour, LL.D., F.R.S., Professor of Natural Philosophy. *Owens College.*

1863, Oct. 6. Stretton, Bartholomew. *Bridgewater-place, High-street.*

1814, Jan. 21. Stuart, Robert. *Kirkennan, Dalbeattie, N.B.*

1870, Mar. 22. Teale, James. *Springfield, Sale.*

1873, April 15. Thomson, William. *Royal Institution.*

1860, April 17. Trapp, Samuel Clement. 88 *Mosley-street.*

1861, April 30. Vernon, George Venables, F.R.A.S., F.M.S., F. Anthrop. Soc., Mem. Met. Soc. Scot., and Met. Soc. France. *Auburn-street, Piccadilly.*

1873, Nov. 18. Waters, Arthur William, F.G.S. *Woodbrook, Alderley Edge.*

1873, Nov. 4. Watkins, James. *Hurst Bank, Heaton, Bolton.*

1874, Dec. 15. Watson, Morrison, M.D., Professor of Descriptive Anatomy, Owens College. *Owens College.*

1874, Jan. 27. Watts, John, Ph.D. 21 *Brown-street.*

1857, Jan. 27. Webb, Thomas George. *Glass Works, Kirby-street, Ancoats.*

1858, Jan. 26. Whitehead, James, M.D.. M.R.C.P. Lond., F.R.C.S. Engl., L.S.A., M.R.I.A., Corr., Mem. Soc. Nat. Phil. Dresden, Med. Chir. Soc. Zurich, and Obst. Soc. Edin., Mem. Obst. Soc. Lond. 87 *Mosley-street.*

1839, Jan. 22. Whitworth, Sir Joseph, Bart., F.R.S. *Chorlton-street, Portland-street.*

1859, Jan. 25. Wilde Henry. *Mill-street, Ancoats.*

1859, April 19. Wilkinson, Thomas Read. *Manchester and Salford Bank, Mos' y-street.*

1874, Nov. 3. Williams, William Carleton, F.C.S. *Owens College.*

1851, April 29. Williamson, William Crawford, F.R.S., Professor of Nat. Hist., Anat., and Physiol., Owens College, M.R.C.S. Engl., L.S.A. *Egerton-road, Fallowfield.*

1871, Oct. 31. Winstanley, David. *Doctor's Cottage, South Shore, Blackpool.*

1836, Jan. 22. Wood, William Rayner. *Singleton Lodge, near Manc' ester.*

1860, April 17. Woolley, George Stephen. 69 *Market-street.*

1863, Nov. 17. Wort' ington, Samuel Barton, C.E. *Crescent-road, Cheetham Hill.*

DATE OF ELECTION.

1865, Feb. 21. Worthington, Thomas. *Bridgewater-Club Chambers, King-street.*

1864, Nov. 1. Wright, William Cort, F.C.S. *The Springs, Bowdon.*

N.B.—Of the above list the following have compounded for their subscriptions, and are therefore Life Members:—

Brogden, Henry.
Sandeman, Archibald, M.A.
Smith, Robert Angus, Ph.D., F.R.S.

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